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NOTICES OF The Aëronautical Society of Great Britain.

At a Council Meeting of the Aëronautical Society of Great Britain, held at 53, Victoria Street, Westminster, on Monday, February 17th, 1908,

1.—The following gentlemen were elected Members of the Society:—

MR. G. M. HERRMANN.
MR. W. V. GILBERT.
LT. J. W. M. MCCOWEN, R.N.
MR. R. MORCOM.
MR. R. B. WOOSNAM.
MR. J. H. LEDEBOER.

To date February 17th, 1908.

2.—It was decided to send a vote of thanks to Major B. Baden-Powell, late Scots Guards, on his retirement from the Presidency of the Society.

53, Victoria Street,
Westminster,
February 17th, 1908.

MY DEAR BADEN-POWELL,

I am requested by the Council of the Aëronautical Society of Great Britain to convey to you the thanks of the Society for the valuable services you have rendered during your seven years' term of office as President.

On behalf of the Society allow me to give you our thanks for the work you have done, and to express our pleasure that, as a Member of the Council, we shall still have the benefit of your advice in matters affecting the welfare of the Society.

Believe me,
Yours very truly,
(Sd.) FRANCIS TROLLOPE, LT.-COL.,
Vice-President.

3.—It was decided to send a vote of thanks to Mr. Eric Stuart Bruce, M.A., on his resignation from the post of Hon. Secretary of the Society.

53, Victoria Street,
Westminster,
S.W.,
February 17th, 1908.

MY DEAR BRUCE,

I am desired by the Council of the Aëronautical Society of Great Britain to express their most sincere and deep regret that you have decided in view of your increasing private affairs to resign the office of Hon. Secretary to this Society, for you have during your term of office, which practically covers seven years, brought the Society from a comparative state of penury to that of comparative affluence. On behalf of this Society allow me to express our most grateful thanks for your great past services, and to express a hope that you will still continue to aid our work by continuing on the Committee of the Society.

The Council beg me to thank you for your generous gift of furniture to the office.

Believe me,
Yours very truly,
(Sd.) FRANCIS TROLLOPE, LT.-COL.,
Vice-President.

4.—It was decided that in future the financial year should date from January 1st to December 31st of each year, and that the

accounts of the Society should be made up to December 31st.

5.—It was decided to form a Committee to consider the disposal of the grant given by the Mercers' Company "towards experiments." The composition of the Committee to be as follows:—

Mr. Dines, F.R.S.
Dr. Shaw, F.R.S.
Col. J. Capper, R.E., C.B.

The Vice-President to act as Final Adjudicator.

6.—The following awards for the Milan Exhibition were made:—

Bronze Medals.

Mr. Eric Stuart Bruce, for his services in connection with the Exhibition.
Mr. S. H. Salmon, for his exhibit at the Exhibition.

At the Council Meeting of the Aëronautical Society of Great Britain, held at the Royal Society of Arts, John Street, Adelphi, on March 27th, 1908,

1.—The following gentlemen were elected Members of the Society:—

Mr. G. F. Leake.
Capt. H. J. Coningham.
Mr. C. Parker.
Mr. J. D. M. Tinline.

To date March 27th, 1908.

2.—The Committee appointed to consider the disposal of the Mercers' grant reported the following awards:—

Major B. Baden-Powell ...	£30
Mr. J. Weiss ... ... (loan for motor, the property of the Society.)	£30
Major Moore, R.E., President of the Committee on "Wings"	£30
Mr. G. P. Saul	£10
Mr. R. Draper	£5
Total	£105

The Council decided that the following instructions should be sent to each of the above-mentioned Members.

1st.—A preliminary report to be sent in by them giving an account of the work done, not later than June 1st, 1908.

2nd.—A final report giving a full account of their work, to be forwarded by them not later than December 15th, 1908.

The Council also suggested that it would be a great advantage if the reports were accompanied by photos, sketches, etc.

MISCELLANEOUS NOTICES.

1.—The second meeting of the 43rd session of the Aëronautical Society of Great Britain will be held at the Royal United Service Institution, Whitehall, on May 27th, 1908, at 8 p.m.

Representatives of the principal foreign Aëronautical Societies have been invited to attend this Meeting, as also the Members of the F.A.I. Congress, then visiting this country.

2.—The Royal Aëro Club of Spain is organising a balloon race for the Municipality of Barcelona, to take place on May 17th, 1908. Entries to be made not later than midnight on May 1st, 1908, to the Secretary of the Royal Aëro Club of Spain, Madrid.

Entrance fee 300 francs.

Intending competitors can obtain further particulars from the Hon. Secretary of the Aëronautical Society of Great Britain.

3.—The following books and publications have been presented to the Library:—

By the Meteorological Office.
Publications of the International Commission for Scientific Aëronautics.

By Capt. Ley, R.E.
"The Possibility of a Topography of the Air, Based on Balloon Observations with Special Theodolites."

By Col. F. Trollope (*Vice-President*).
Miscellaneous Publications.

By Messrs. Constable.
"Aërodynamics," by Lanchester.

4.—The following is a list of the prizes now offered for flying machines, etc., from "La Conquete de l'Air," March 15th, 1908.:—

A.—FLYING MACHINES.

Belgium.—At Spa, on July 5th, 12th, and 19th, 1908, 75,000 francs in prizes, etc. (Aëro Club of Belgium).

France.—Armengaud Prize, 10,000 francs, for the first aviator who succeeds in remaining in the air for 15 minutes.

Pepin Prize of 1,000 francs to first aviator who crosses the River Garonne. Aëro Club of France Prize of 500 francs for the best level suitable for aëronautical work.

Aëro Club of France: Three prizes of 200 francs, and 3 silver medals, for flights of 200 metres.

Archdeacon Cup, at present held by Mr. Henry Farman.

Ruinart Prize: 12,500 francs, for first aviator who flies from France to England, or *vice versâ*.

Aëro Club of France, 5,000-metre flight. Prize of 5,000 francs.

Michelin Prize No. 1: An "objet d'art," valued at 10,000 francs, to be competed for under rules laid down by the Aëro Club of France. In addition 15,000 francs go to the driver of the machine, and this will be continued for 10 years.

Michelin Prize No. 2: 100,000 francs for a flight (two persons in the machine), from any point in the Departments of the Seine, or Seine-et-Oise, to the summit of the Puy de Dome, the Arc de Triomphe, and the cathedral at Clermont Ferraud to be circled en route.

Great Britain.—"Daily Graphic" Prize of £1,000 for a flight of one mile.

"Daily Mail" Prize of £10,000 for a flight from London to Manchester. In connection with this there is a special prize of £2,200, given by Adams Manufacturing Co., and a gold medal offered by Mr. Santos Dumont.

Germany.—Dr. Gau's Prize at the Munich Exhibition, £500, for a flight in the air lasting 10 minutes.

B.—FOR PILOTS OF DIRIGIBLES OR FLYING MACHINES.

Deutsch Prize: A Cup valued at 10,000 francs, and three prizes of 20,000 francs each. Rules for this competition are laid down by the F.A.I.

C.—FOR PILOTS OF SPHERICAL BALLOONS.

Belgium.—Aëro Club of Belgium: The Paris-Brussels Cup.

Aëro Club of Belgium Cup, given by l'Etoile Belge (at present held by Mons. de Bronckère).

France.—Gordon-Bennett Cup, valued at 12,500 francs, and prize of same value (at present held by Captain Erbslöh).

Santos Dumont Prize of 4,000 francs for the aëronaut who remains 48 hours in the air without descending. Cup given by le Gaulois (at present held by Mons. Leblanc).

America.—Lahm Cup (at present held by Mr. Mac-Coy).

Italy.—The Margaret of Savoy Cup (at present held by M. Uselli).

5.—Mr. P. Y. Alexander has presented his Aëronautical Library (which includes among other works 70 volumes of Press cuttings) to the Victoria and Albert Museum. Members desirous of inspecting the books should apply to Mr. Fulcher at the Museum.

6.—The Press cuttings supplied to the Society during March Quarter, 1908, are now available for issue to Members. Any Member who wishes to have them should apply to the Hon. Secretary on April 30th, 1908. They will be given to the first applicant.

GENERAL MEETING.

The opening Meeting of the 43rd Session of the Aëronautical Society of Great Britain, was held at the Royal Society of Arts, John Street, Adelphi, on March 27th, 1908. The chair was taken by Major B. Baden-Powell (late Scots Guards).

The CHAIRMAN: The first paper this evening will be by Mr. Parsons. I have much pleasure in calling upon him to give us his paper.

Mr. W. B. PARSONS, M.R.C.S., then read a paper entitled "Some Circumstances Attending the Rotation of Planes in Air, with Special Reference to Aërial Screws."

Deductions from Experiments

SOME CIRCUMSTANCES ATTENDING THE ROTATION OF PLANES IN AIR, WITH ESPECIAL REFERENCE TO AËRIAL SCREWS AND PROPELLERS.

A Paper read before the Aëronautical Society of Great Britain on March 27th, 1908.

By W. B. Parsons, M.R.C.S., &c.

Mr. President, Ladies and Gentlemen,—In bringing before you the results of my experiments on the rotation of planes in air, it is but fair to say that they were undertaken two or three years ago, at a time when I was ignorant of the conclusions of other investigators and of the opinions prevailing amongst those who have the best right to speak authoritatively on the subject.

My experiments were undertaken more as a pastime than in any spirit of earnest aërodynamical investigation, and, as is to be expected under such circumstances, I spent a great deal of unnecessary time and trouble in investigating questions already definitely set at rest by the work of more accomplished observers. In so far as certain of such questions are capable of being completely answered by direct experiment, the information in regard to them is already in our possession, and where I find my results in agreement with accepted facts I will not trouble you with them. There are, however, certain questions in which I find myself unable to agree altogether with accepted authority, my own conclusions being in conflict therewith, and, also, there is a curious coincidence in the relation between the angle of inclination of a rotating plane and the velocity acquired under a constant force, which I can claim to have noticed, and which if it be found to obtain in the motion of larger planes impelled by greater powers than I have worked with, would form at once a simple and direct means of co-relating angle and velocity.

Present endeavour to compass aërial sustentation and flight is directed towards the aëroplane as against the aërial screw. There is no doubt that the writings and experiments of Dines and Langley are largely responsible for the tendency, together, of course, with the practical results achieved and reported to have been achieved with the aëroplane. There are, however, signs that indicate that the aërial screw is beginning to receive its meed of attention, and our notice has been drawn to an important paper communicated to the French Academy of Sciences by Lieut. Vlahavos, of the Greek Artillery, which is a comparative study of aëroplanes and bladed air propellers. The conclusion is that the aëroplane has no advantage over the screw.

I think it may fairly be stated that everybody is agreed that we have in the aërial screw, driven by present-day engines, all that is necessary to accomplish aërial sustentation, and that the difficulties in the way of aërial navigation by means of screws to lift and propel are chiefly mechanical. But experiments in this field have been deplorably few. Our information is meagre and in some instances misleading, and much remains to be learned concerning the best form to be given to aërial screws, the proper area, velocity, and pitch, as well as the power required for sustaining or propelling a given weight in the air with a screw.

It has been my object to obtain some reliable information on each of the points referred to.

(After a brief account of aërodynamical principles connected with the subject, in the course of which Mr. Parsons referred to Duchemin's formula which reaches its maximum at 36°, and De Louvrié's rational formula with its maximum at 39°, Mr. Parsons continued:)

I will without further introduction proceed to describe the nature and scope of my experiments.

The photograph which I exhibit will serve to make clear the arrangement of my apparatus.

It consisted of an electro-motor, connected through lamps with a source of electricity.

The motion of the motor is communicated to a vertical spindle in ball bearings, upon the summit of which is a bed to accommodate the screw.

The screw is constructed of a shallow cup-shaped boss made of brass, to opposite sides of which are fastened holders for taking the blades used in each experiment.

The cup is intended to receive flat circles of brass, so that the weight of the screw could be increased when desired without adding to the surface of the blades. The blades are made of a framework of stiff wire covered with silk and, in form, are sectors of a circle.

Of these I had five pairs of different size carefully weighed and equalised. They could be firmly fixed to the boss at any desired inclination.

A spirit level mounted on the board carrying the motor and spindle ensured horizontality in working.

A circular scale of angles, situated in a convenient position and carefully adjusted so as to be level and vertical, enabled me to set the blades.

Within the uprights supporting the spindle can be seen a clockwork instrument which carries a running strip of paper about ½-inch in width, and in a corresponding position on the other side a holder carrying a compressed tabloid of fuchsin. Attached to the spindle, but not seen in the photograph, is a pointed quill, which revolves with the spindle and, striking alternately the fuchsin and the running paper, records the revolutions of the spindle and so of the screw. The markings when made are so faint as to be hardly discernible, but upon wetting the paper they come up with surprising clearness and sharpness.

This recording arrangement is the outcome of many hours of fruitless labour. I had tried writing arrangements actuated by springs, by levers, electro-magnetically, and in other ways, only to find that whilst making very beautiful records of low velocities they were quite useless for higher velocities, owing to delay in the writing due to mechanical inertia and vibration.

Time was measured by means of a stop watch, and in some experiments by means of a writing metronome carefully corrected. In the case of a stop watch there is the disadvantage of the personal element, but a little practice in working and the repetition of a doubtful experiment were sufficient to neutralise any error on this score.

There is also to be seen in the diagram a stand bearing two pith balls depending by threads from arms movable along a scale, the use of which I will point out later.

Everything being in readiness, I set the motor in motion with the screw blades at right angles to the direction of motion, using for blades, sectors of 45° included angle, and I obtained a record of revolutions for that particular power of motor for 10 seconds.

I took similar records with the blades inclined at angles 80°, 70°, 60°, 50°, 40°, 30°, 20°, 10°, and 0° respectively, with the motor running under the same power and all connections undisturbed. I thus obtained a series.

The angle of inclination was confirmed after each experiment, and for one reason or another many experiments had to be repeated several times in order to obtain a complete series.

I took many score of series at different powers of the motor, but any series complicated by a necessary re-adjustment of machine was left incomplete or rejected altogether.

I was thus enabled to note the behaviour of the screw at different inclinations of blade, but under the same conditions of motive force and friction.

It is obvious that for any given power the only variable factor present is the inclination of the blades. The consequent variation in velocity is the expression of the air resistance for that inclination and velocity.

Desiring to maintain a mind free from preconceived ideas I studiously avoided consulting any authority on the value of air resistances, nor did I do so until the moment of preparing this paper.

As the most clear and simple method of exhibiting the variation is by means of a graph I have plotted the curve of a set of revolutions.

(These are reduced to angular velocities in feet per second for comparison with Wolff's table of wind pressures.)

Rev. ...	30	33	33	43	44	55	67	84	113	165

It is simple to at once detect and exclude any value which, by its position in the graph, is obviously incorrect.

Having obtained a curve which, upon a cursory glance, seems to belong to the conic sections, a figure one is naturally led to expect, I endeavoured to find a mathematical expression for it, but the finding of a curve which shall pass through a finite number of isolated points has an infinite number of solutions, because there is no restriction whatever on the form of the curve between those points.

The curve of lowest degree which can be drawn through the nine points is evidently one containing 27 constants and is of the 26th degree. To find it would involve an immense amount of useless labour, and as it is not calculated to advance the problem which I had set myself to solve I will neither attempt nor will I ask you to follow me in any solution of it.

I determined to resign a wearisome task, and turned my attention to the estimation of the best angle of inclination for the blades.

You will recollect that other things being equal, and consideration for the moment being withheld from any other factor, the resistance depends on the sine of the angle of inclination. I do not mean to say that it depends simply upon the sine, but it is certain that this is a factor of importance, and the inverted curve of the natural sines of the angles is the form that the graph should assume if this factor were free to act alone. I would like to make it quite clear that as yet I postulate nothing for the experimental curve of velocities. I merely refer it to quite arbitrary axes. This is, indeed, necessary in view of what I shall have to say later.

Plotting the curve of sines to correspond with the experimental curve it is easy to see the divergence between them, and by subtraction to find where the difference is greatest. It is obviously here that the product of the other disturbing factors is most in evidence.

It is here that the experimental curve is most forced away from the path which it would take if the only factor present were the effective surface of the plane, varying as the sine of the angle of inclination.

It is here that in proportion to surface the air pressure is greatest.

The greatest difference is seen to lie at about 22° of inclination.

This is, without any doubt, an unscientific, unmathematical, and altogether inexcusable process. It is nothing more nor less than a juggle.

I feel that an apology and an explanation is necessary. I have preferred to invert the curve of the sines in actuality on the graph and so preserve the reality of circumstance.

Without the statement that I have made it would be difficult to explain how I was led to choose this angle of 22°, for it was neither the result of chance nor of a direct examination of each inclination through 90 degrees.

It is, moreover, a deduction from a particular case, and cannot be made of general application.

Nevertheless, it is amply confirmed experimentally.

I fitted my motor with a brake upon the driving wheel, actuated by a micrometer screw, by which I was able to obtain the utmost nicety of adjustment of the power exerted by it.

I inclined the screw blades to about 22° and obtained the motive power which was just sufficient to raise the screw. I found this to be insufficient at 30° of inclination, or any higher angle, and insufficient at 20°, and by many repeated experiments I confirmed the conclusion.

I then substituted for the motor an arrangement of weights and pulleys by which to drive the screw. It was necessary to interpose at least six pulleys, and I did not find the method nearly so delicate as the former.

I found, however, that a weight which was just sufficient to cause the screw to rise with blades inclined at 22° was insufficient to lift it with blades inclined at 30° or at 20°.

Angle.	Current to Motor.	Braked.	Revols.	Position of Screw.
22°	32 c.p.	Yes	160	Mid-elevation.
30°	32 ,,	Free	—	Did not rise.
30°	37 ,,	Yes	117	Mid-elevation.
45°	37 ,,	Free	—	Did not rise.
45°	48 ,,	Free	123	Mid-elevation.

There is a circumstance particularly striking in the rising of the screw with the blades inclined at the various angles.

Much below 20° I was unable to get the screw to rise at all. At inclinations between 20° and 30° the rising of the screw was free, perpendicular, and even. Above 30° rising was irregular with much side to side rocking, and this irregularity increased with increase of inclination.

Whereas below 30° the screw rose smoothly and gradually, above that angle the rise was sudden and jerky.

At this stage of my experiments I substituted a hand-driven machine for the motor, and by its means I was able to obtain a much greater power. The screw in these experiments was free to rise to any height, and it was simple to throw it up in a vertical direction to the ceiling, with blades inclined between 20° and 30°, but when the inclination was greater the screw took an oblique path, the direction of which it was impossible to foretell. I did not continue these experiments as my screw had some narrow escapes from being damaged.

Circumstances now arose which led me to abandon my experiments for a time, satisfied that I had found nearly enough for practical purposes the best angle of inclination for the screw blades.

On resuming the investigation a year later in June, 1907, a careful scrutiny of the values between 20° and 30° led me to believe that an inclination of 21° was probably the point of maximum utility for lifting, and to form certain conclusions in regard to the curve of velocities the consideration of which I will defer for a while.

The next point to decide was whether or not the inclination should be varied from periphery to centre as is usual with propellers and fans of all kinds.

It is authoritatively stated that there should be a uniform total pitch for all parts of the screw, *i.e.*, a decreasing rate of pitch from the centre towards the outside.

The late Mr. Hastings, in a paper published in the Proceedings of the International Conference on Aërial Navigation, held in Chicago in 1893, suggests that if this principle be carried out that portion of the surface of the screw which is set at a greater angle than 35° (the maximum by Duchemin's formula) is peculiarly inefficient, and that the exposure of this portion of the surface should be avoided, and the central portion so shielded that no portion of the blades the angle of which is greater than that of maximum thrust should be exposed.

A large available surface is thereby sacrificed.

In the case of windmills the inclination of the sails is varied in order to allow of the more slow escape sideways of that part of the sail which is travelling slower than another, the impulse and velocity of the wind being the same at all parts.

There is no reason for supposing that in the case of screws the same considerations apply. Just as the angular velocity of a screw varies from a maximum at the periphery to zero at the centre, so does the wind produced by it vary in the same proportion from periphery to centre, and there is no *primâ facie* reason for supposing that an inclination which is best at a particular velocity is not best at any other velocity.

My experiment to decide this question was as follows:—

I procured a pith ball and suspended it by a strand of silk from an arm attached to an upright, in such a position that I could successively bring the ball over any desired portion of the blade while in revolution, and by means of a scale along which the arm moved freely I could read its position in regard to the moving blade at any moment.

The arm also carried a vertical scale of angles by means of which the deflection of the pith ball occasioned by the draught could be measured.

I found the point of maximum pressure to lie over a section of the blade $\frac{5}{6}$ of the distance from the centre. This was to be expected, seeing that at this spot the blade was widest, but what I did not expect to find was that, whereas the pressure on the pith ball seemed to decline gradually as the ball was moved nearer to the centre, it fell very suddenly as the ball was moved further out from the maximum point, and became lost opposite the extremity of the blade. There is, therefore, an area in the blade of a revolving screw, which, compared with the centre of pressure and the rest of the surface, is a very ineffectual area, an area that might be sacrificed without much loss to the effective power of the screw.

The inclination of a blade at this point could be reduced as low as possible without sacrifice of power. The gain of a smaller inclination at the extremity is incalculable seeing it is here that the velocity is greatest and the effective surface least.

I will refer to an accident which befell at an earlier stage in my experiments, which, indeed, led me to make the experiment just described.

In attempting to affix with sealing-wax a writing point to the extremity of one of my screw blades (a method of recording velocities which I subsequently discarded in favour of the plan described above), I brought the flame of the spirit lamp too near the silk covering, with the result that the sealing-wax, catching fire, a hole was burned in the extremity of one blade.

It was inconvenient to replace the covering that day, and as I was about to record the number of revolutions necessary to lift the screw, I proceeded with the trial, making several records.

I obtained as a result from 90 to 100 revolutions per 10 seconds. After the repair of the blades I was never able to raise the screw under 120 revolutions in 10 seconds.

I should refer in this place to Professor Langley's observation, of which I was then unaware, viz., that holes may be cut in the planes without reducing the aggregate pressure in proportion to the surface cut away.

The conclusion, then, is that although there seems no reason for altering the inclination of the screw blades from the point of maximal pressure towards the centre, the advantage of lessening the inclination from the maximal point peripherally is incalculably great, and that this should be done as sharply as mechanical possibilities will allow.

The next question has regard to the area of the blades. In a very instructive article on fundamental principles, by Rankin Kennedy, contained in the "Aëronautical Journal" for January of this year, it is pointed out that the energy required to lift a pound weight in air by means of a screw is not a fixed quantity, but depends on the speed of revolution and the weight of fluid acted upon. There is no limit to the variation of these terms in theory, but in practice the area of the propeller blade soon becomes unmanageably large when the speed of revolution is kept low, and with much increased speeds the waste of energy becomes prohibitive. There is a middle course and a best area for a given power.

For a decision on this point I had constructed five pairs of blades.

A pair of semi-circles.
A pair of sectors of included angle 120°
A pair ,, ,, ,, 90°
A pair ,, ,, ,, 60°
A pair ,, ,, ,, 45°
all having the same radius.

The semi-circular pair were useless. They tended to make the screw behave as if it were two attached planes to tilt up one side immediately it commenced to revolve, after which the screw would not regain a horizontal position, but remained in a condition of great instability.

Results obtained with the other surfaces are as follows:—

Sector.	Area (inch).	Weight (Grms.)	Added Weight.	Power	Proportion of Circle of Revolution.	
120°	38·72	576	Nil.	Total Weight Carried, 576. Constant, 56 C.P.	— Circle of Revolution, 82·46.	1
90°	21·66	548	28		·38	2
60°	18·90	548	28		—	3
45°	11·20	448	100		—	4
45°	11·20	448	28	476	—	5

Remarks:—
1.—Does not rise.
2.—Remains suspended.
3.—Does not rise.
4.—Does not rise.
5.—Just rises and remains suspended.

So that it will be seen that of these areas the most effective is one which fills a little more than one-third of the area of the circle of the screw, which agrees well with Wenham's estimate. It will also be noted that the use of a sector of 45° included angle required the same power to lift and keep suspended a screw 100 grains lighter than that provided with blades of area $\frac{1}{3}$ of the circle, the area in the former case being about $\frac{1}{7}$ of the circle of revolution.

(It should be explained that the sector whose included angle is given has reference only to the blade and not to the circle of revolution, whose diameter is considerably greater.)

The next question for decision is the number of blades over which the most effectual surface should be distributed. To make direct experiments would have entailed a complete re-construction of my screw and spindle, an undertaking that I did not consider worth while, as I have from other data concluded that a two-bladed screw is the most effectual. I am well aware that a three-bladed screw has been recommended, but I have not heard a satisfactory reason given for its use. On the other hand, it has been found that multiplicity of blades tends to inefficiency of action.

In Sir Hiram Maxim's experiments in the propulsion of inclined planes it was found that a two-bladed screw gave the best results.

In the Copenhagen experiments it was shown that not only would the aërial propeller develop as great a thrust as the water propeller in proportion to the energy consumed, but that under certain conditions it would do slightly more, and greater thrusts per horse-power were obtained than in any previous experiments. A two-bladed screw was used which was calculated to do work equal to 63 per cent. of the energy imparted to it, and it was further noted that the power was augmented in 75 per cent. of the directions from which the wind could blow.

Professor Langley, in some experiments for arriving at the power available for moving his planes, used a six-bladed propeller with blades set at 45°, and also tried propellers of four and eight blades, but discarded them in favour of an aluminium propeller of two blades set at an angle of 75° with the axis.

It is reported, with how much truth I am, however, unable to say, that the battleship "Cæsar," when examined in dock at Devonport, was found to have lost one of the blades of the starboard twin screw. The mishap was by no means recent, the main engines had been run since the breakage happened, yet nothing in the action of the machinery gave any indication of it. An incident is recalled of the battleship "Renown," which made a record passage from Bermuda to Devonport at an average speed of more than 15 knots. When docked on arrival it was found that she had performed this feat with one propeller blade knocked away, yet there was no loss of driving power or increase in the mean slip of the screws.

When in my experiments it became necessary to allow the screw to rise off its bed, and I was registering revolutions directly from the screw blade, I was compelled to seek a means which would be uninterfered with by the rising screw.

I adopted the plan of fixing a light wooden pointer to the bed of the screw, the pointer projecting beyond the blades and at right angles to them and nearly in the same horizontal plane.

When everything was ready, and my motor working braked down to the previously ascertained lifting point, the screw did not rise.

In seeking for the cause I inadvertently brought my hand into the path of the revolving pointer, with the result that the pointer broke off short, and the screw immediately rose.

Thinking that the weight of the pointer might have reduced the motive power below the lifting point, I substituted for it a short file many times heavier than the pointer when on setting the motor in motion the screw rose despite the additional weight.

I then fashioned and fixed another pointer, and again found the interference present.

I repeated this experiment many times with the same result, but on the following day I failed to produce it several times, and several other times I succeeded in reproducing it.

It seems that the presence of the pointer in the path of the blades and at right angles to them in some way seriously interferes with their effectiveness, the interference occurring in a space of very limited vertical dimension.

I have devised some experiments for investigating this most curious circumstance, but I have not yet had an opportunity of carrying them out.

It is, however, sufficient to indicate, and I think the conclusion may fairly be drawn that a third or fourth blade, occupying as it would be certain to do, this critical position in the path of revolution, would insomuch detract from the effectiveness of the propeller.

The next question for investigation was the nature of the surface. I remember having heard, although I cannot recall from whom or when, an observation that seems full of significance. A bird that had just passed through a thick column of smoke was brought down and an examination of its wings revealed particles of soot between contiguous feathers. It was clear that during flight the air had passed through the spaces between the feathers.

The consideration of this fact led me to try an imbricated surface for the blades of the screw with the following result:—

A plane surface required 120 revolutions per 10 seconds, whereas an imbricated surface required a much greater speed to lift the same weight and size of screw.

A reference should find a place here to Professor Langley's experiment in which he found that inclined planes may be superposed without diminishing the sum of their separate individual pressures, provided they be not too close, but even in this case the amount of interference will vary with the speed.

With a hole cut in the covering of each blade just outside the point of maximum pressure I obtained a distinctly better result in the power required to lift the screw, and there was little if any difference between the power necessary at 22° of inclination and that necessary at 30°, but on enlarging the holes there was a distinct falling off, and I was led to conclude that there is no superiority in a screw with holes in the blade over one with the inclination beyond the point of maximum pressure lowered, as I have already explained.

This leads me to an observation on the occurrence of "bulging."

Although there is little that is common in the action of windmills and aërial screws, there is this in which they agree. They both depend for their action on the pressure of the air upon surfaces inclined obliquely, and the circumstances which are in operation in the one case to produce a point of

maximum effect are the same in the other case.

The best angle of inclination in both cases should, therefore, be the same if what I hope to show in regard to the law of air resistance be true.

That the best angle of inclination for the setting of windmill sails is not 22°, but a much lower one, 18°, is amply proved by the experiments of Smeaton and all who have followed him.

I attribute this apparent discrepancy to the occurrence of bulging in the windmill sail, and I have confirmed my opinion experimentally.

With the silk covering of one blade slack and of the other taut, I found it necessary to incline the slack blade at about 15° whilst the other remained at 22° in order to obtain an even rise of the screw, and to compensate for the bulging.

With both frames at 22° there was considerable side to side rocking, which always occurs with unequal inclination of the blades, particularly at the higher angles.

I next tried the effect of a porous surface, thinking that the resistance to the passage of air through numerous small holes might be enough to compensate for the loss of area incurred, whilst at the same time there would be a gain in lightness of the blade, but my supposition was not borne out as I found these blades much inferior to any I had previously used.

Corrugation of the surface is a means for increasing the area without increasing the radius of the screw, and is supposed to be of use in reducing skin-friction.

Professor Langley has pointed out that skin friction as an accompaniment of the movement of planes in air is largely imaginary, and Sir Hiram Maxim found it to be a quite negligible quantity. I have, therefore, no reason for supposing that a corrugated surface is likely to prove superior to a plane surface of the same extent, but I did not experiment with it.

In a work recently published Mr. Lanchester has called in question Professor Langley's statements in regard to skin friction, and states that an allowance of 2 per cent. should be made on account of it.

To decide the velocity with which the screw must be revolved and the power required to lift it, it was only necessary to make an estimation of the force expended in terms of H.P. whilst the screw was at what I may call the middle elevation, *i.e.*, neither rising nor falling.

It was easy to obtain any desired rise within the limits of my safety catch and maintain the screw indefinitely at that height by regulating the speed of the motor to a corresponding degree.

For velocity, the results with stop watch and with metronome are subjoined in the table, the blades being inclined at 22°, the best angle.

Weight of Screw.	Time.	Revolutions.	Power.	Remarks.
448 grs.	20·5 $\frac{200^m}{200^m}$	80 13 per sec.	8 lbs. 10 ozs. Over 6 pulleys	—
—	20 sec.	240 12 per sec.	Motor	Just lifted.
—	20 sec.	360 18 per sec.	Motor	Well up at top.
—	20 sec.	304 15·2 per sec.	Motor	Well sustained.
548 grs.	10 sec.	137 13·7 per sec.	Motor	Mid-elevation.

Wenham found a force of 60-foot pounds per minute requisite to raise 396 grains, or in the ratio of 3-H.P. for every 100 lbs. Experiments on a larger scale have demonstrated that one H.P. can sustain a weight of 33 lbs., which agrees very well with Wenham's estimate. I am not acquainted with the details of these experiments, nor can I say what was the inclination of the screw blades or the pattern of screw used.

It is well known that the calculation of forces expended upon models of the dimensions of my screw are likely to be inaccurate however carefully done, but the error is usually in excess of power. Practical engineers state that a model of a machine which will yield but 20 per cent. of the power expended upon it will usually yield 70 per cent. when made up in full dimensions.

I put forward my estimation with some hesitancy on account of the large probable error, but in view of the fact just stated, with less concern than otherwise.

Comparing it with Wenham's estimate it shows a marked advantage.

Having obtained the power necessary to lift and maintain the screw at middle elevation, by braking down the motor to the proper degree, I removed the screw from its bed, leaving all other connections undisturbed, and made an estimation of the power by means of weights hanging over a pulley on the driving wheel.

The figures are as follows:—

Power Estimation.—Blades .38 of area of circle of revolution, inclined at 22°.

Revolutions of screw at mid-elevation	No.	137
„ „ Motor weighted	No.	178
Diameter of pulley	ins.	0.9
Time	secs.	10
Area of blades	s. ins.	21.66
Load on spindle (diff. of weights) ...	grains	1620
Weight of screw	grains	543

Space travelled over $= 17{\cdot}8 \times 9\,\text{II}$ ins. per sec.

$$\text{Work} = 17{\cdot}8 \times 9\,\text{II} \times 1620 \times \frac{60}{12} \text{ gr. ft. min}$$

$$\frac{548 \times 33000}{17{\cdot}8 \times {\cdot}9\,\text{II} \times 1620 \times 5} = 46.45 \text{ lbs. per H.P.}$$

Log.		
		.497,149
2.738,781		1.250,420
4.518,514		1.954,243
7.277,295		3.209,515
5.610,297		.698,970
1.666,998	Log. 46.45	5.610,297

I also undertook several experiments to determine the condition of the superincumbent and underlying air, and the direction of air currents, during the revolution of the screw both when held down and when free to rise.

Experiments with a pith ball suspended vertically above the centre of the screw showed that the air above the reversed screw was twisted into a cone the base of which occupied the path of the screw, and the apex varied with the inclination of the blades.

At 90° and 80° the pith ball suspended a radius' distance above the screw was unaffected, at 70° it began to oscillate slightly, at 60° the ball described a circle of $\frac{2}{10}$ inch diameter, at 50° this reached $\frac{4}{10}$ inch, at 40° $\frac{5}{10}$, at 30° 2 inches, at 20° $1\frac{4}{10}$, and at 10° it fell again to $\frac{4}{10}$.

The screw is apparently lifted up in a veritable whirlwind. I would draw your attention to the fact of the maximum effect between 20° and 30° of inclination, the force of the draught bearing a similar relation to the angle of the blade as in lifting, a further confirmation of the superiority of this angle.

It has been pointed out by writers of authority that there is much confusion of mind prevalent in regard to screws when used as fans and when used as propellers, the most effective propeller being that which produces the least disturbance in the fluid in which it operates and the most effective fan that which produces the greatest.

If a screw were free to move in a resisting fluid without slip the effect of revolving it would be a motion through the fluid of the screw, but if the screw be restrained from advancing the result is a motion of the fluid and total slip of the screw.

If the screw have any work to perform it is to that extent fixed, and in that proportion will it slip.

It does not follow that the slip at every inclination of blade will be the same. If the screw revolve at such a velocity that the time of one revolution is equal to the time of one slip, it is clear that the screw will not advance at all, but if the screw be made to bite faster than it can slip it must progress. It does not follow that the biggest bite is the best. At a given power there is a very definite relation between the bigness of the bite and the quickness of it. It is inverse, but not in simple proportion, there is a maximum and a minimum point, the maximum lying as is shown at about 22° of inclination. The nature of the relation I hope to deal with presently.

In the Copenhagen experiments a very curious circumstance occurred in which what is called negative slip was demonstrated.

At a speed of 70 revolutions per second the screw rose between 30 and 100 feet in height in the air in less than a second, when, according to pitch (which was 1 foot), it should have required one second to reach this height if there had been no slip at all.

It is clear that the screw was lifted up by some force in addition to that obtained by its revolution and reaction against the air. It may be, and possibly is, that at such velocities as 70 revolutions per second other factors come into account which have no place at a more economical speed.

There is a prevalent notion that when a screw rises in air it, as it were, compresses the air below it and sucks in the air above, the diminution of pressure above and the increase of pressure below combining to lift it. This view is, indeed, strengthened by the Copenhagen experiments, H. C. Vogt stating that "propellers both in air and water do their work by causing a rarefaction, *i.e.*, by diminution of pressure or partial vacuum on the drag or rear side of their blades," nearly the whole thrust resulting from this rarefaction, which amounts to a difference of pressure of several *inches* of water.

Again also it may be that these occurrences take place at such high velocities as 70 revolutions per second, but at the velocities at which I have worked this does not obtain.

I introduced one end of a mercurial manometer into the air space both above and below the revolving screw (velocity of about 20 revolutions per second) whilst the other end was sufficiently far removed to be outside the area of influence. I was not able to detect the slightest difference in pressure. Seeing that by simple suction with the mouth I could cause a very considerable inequality in the heights of the mercury limbs, I am forced to conclude that there is no actual inequality in these pressures at velocities somewhat in excess of those sufficient to lift the screw.

We know that cavitation occurs in water propellers, and it is not permissible to deny cavitation in air propellers, but the statement that an air screw is lifted up by cavitation and suction due to centrifugal force as concluded from the Copenhagen experiments I am in a position to oppose.

My next experiment was to revolve the screw completely enclosed in a large bell-glass. Within the glass were a number of very small feathers which, being caught in the current of air, indicated its form and behaviour.

With the screw inverted the feathers swept round the walls of the bell-glass, but a few of the heavier were not immediately caught up, but, gaining a position beneath the blades, were rapidly drawn towards the centre and, being then entangled in the current, were whirled round with the rest.

The experiment was complicated by the fact that the air, being confined, was reflected from the floor and walls of the chamber, and although it clearly showed the form of the whirlwind created by the screw, I am not prepared to draw any useful conclusion.

I now ask your attention to the "curve of velocities."

Being tempted during the long evenings of the winter of 1906 to re-examine the curve and seek a solution of it, I tried at random many formulæ, and in despair sought to see how it would agree with a simple Harmonical Progression (*i.e.*, a progression of reciprocals).

I took the series beginning at 30 revolutions and ending at 165, and inserted eight harmonical means between them. I found the progression approximate closely with the figures obtained by experiment (as you see).

Considering the many sources of possible error, remarkable as this approximation was, I could only set it down as a coincidence.

I examined several other series of revolutions, and determined suitable harmonical progressions, being limited in my first and last terms to those obtained on the machine. I was surprised to find the closeness of agreement between the figures calculated and those obtained by experiment. The plotted harmonical curve seems to hug the curve of velocities.

I then obtained a record of revolutions at 90° of 40 in 10 seconds and one at 30° of inclination of 90 revolutions and constructed the following H.P. neglecting fractions:—

40, 44, 49, 55, 63, 70, 90.

I set the blades of the screw at 60° and took a record, using the same power of motor and preserving as far as possible every condition unaltered. Judge of my satisfaction to find the count number 55 revolutions in 10 seconds, as foreshadowed in the progression.

To further confirm this most singular coincidence, for such I must continue to call it, I set up an arrangement of weights and pulleys wherewith to actuate the screw, and obtained the following records:—

36	x	43	46	x	58	72	84	x	x	
90°	80°	70°	60°	50°	40°	30°	20°	10°	0°	

My writing arrangement was not working well, for the new setting up of the apparatus led to a great many annoying complications and necessary adjustments. The records for 80° and 50° were unreadable, and those for 10° and 0° had to be rejected as untrustworthy, owing to limitation of space in the room, which was too small to permit of the use of enough string at the higher speeds, and if a shorter string were used the fall of the weight was noticeably accelerated, which, of course, could not be allowed.

Taking 84 and 36 as fixed points in my H.P., I obtained a series

36, 39.19, 43.01, 47.66, 53.44, 60.81, 70.54, 84,

which agrees very well, despite the many difficulties encountered.

I therefore felt justified in assuming that the velocities of revolving inclined planes bear a harmonical relation, when the angles of inclination are in arithmetical progression.

It was necessary to find a curve such that this relation will hold true whatever may be the common difference and for all magnitudes of the first term.

If the expression for the curve be

$$y = \frac{1}{f(x)}$$

and y_1, y_2, y_3 be the ordinates corresponding to the abscissæ $(x-h)$, x, $(x+h)$, then since the ordinates are in H.P.

$$\frac{1}{y_1} + \frac{1}{y_3} = \frac{2}{y_2}$$

$$\text{or } f(x-h) + f(x+h) = 2f(x),$$

and this relation must hold good for all values of x and h.

By Taylor's theorem this is equivalent to

$$2\left[f(x)+\frac{h^2}{\lfloor 2}f''(x)+\frac{h^4}{\lfloor 4}f''''(x)+\text{etc....}\right]=2f(x)$$

giving

$$f''(x)=f''''(x)=\text{etc.}=0$$

These are all satisfied if $f''(x)=0$

and this condition is necessary since the above relation is to hold for *all* values of h.

Now if

$$\frac{d^2f}{dx^2}=0$$

$$f(x)=ax+b$$

and this is the only possible form for f(x).

Consequently,

$$y = \frac{1}{ax + b}$$

is the only expression for the curve defined as above (where a and b are arbitrary constants.)

The form of the curve could have been easily seen to begin with, but without the functional investigation it would not have been so easy to see that it is the only possible relation between x and y satisfying the given conditions.

It is unfortunate that this does not involve a trigonometrical function.

The figure is obviously a rectangular hyperbola, and it is interesting in relation to Marriott's law of gaseous pressure and density.

The expression found is based upon the assumption of the relations in Harmonical Progression which the velocities assume when the angle of inclination of the planes varies in Arithmetical Sequence.

The variation is seen to prevail in direct experiment, and if the values be not exact they, nevertheless, approach the truth so closely as to make the difference of no practical moment.

It now becomes of interest to enquire if the angle of maximum effectiveness can be found mathematically.

Consider the cases

$$y = \frac{1}{ax + b}$$

$$y = \frac{1}{\sin x}$$

and find the stationary value of the expression.

$$\frac{1}{\sin x} - \frac{1}{ax+b}$$

$$\therefore \quad \frac{\cos x}{\sin^2 x} = \frac{a}{(ax+b)^2}$$

$$\text{or } (ax + b)^2\cos x = a \sin^2 x$$

$$\text{or } \left(x + \frac{b}{a}\right)^2 \cos x = \frac{\sin^2 x}{a}$$

In the progression 30 to 165

$$\frac{b}{a} = 20 \qquad \frac{1}{a} = 3300$$

$$(x + 20)^2 \cos x = 3300 \sin^2 x$$

If these curves be plotted they will be seen to intersect between 68° and 70°.

By means of tables it will be found

when $x=68°$ $(x+20)^2 \cos x > 3300 \sin^2 x$

when $x=69°$ $(x+20)^2 \cos x < 3300 \sin^2 x$

showing that the root of the equation lies between these values.

In several other cases examined, the root is not far from the same value.

It seems to be probable, therefore, that the angle of maximum efficiency is not a fixed and definite inclination, but that it varies between very narrow limits, the variation depending upon the value of the constants *a* and *b* in the expression

$$y = \frac{1}{ax + b}$$

There can be no doubt that there is a relation between *a* and *b*, but the particular values examined have as yet afforded me no clue to it, and it is also probable that the angle of maximum efficiency and the relative value of the two constants are dependent and continuous variables.

I hope on a future occasion to be able to communicate a statement on the point if my conjecture be correct.

The conclusions at which I have arrived may be briefly summarised as follows:—

I.—The power remaining constant the greatest effect is produced when the blades of a screw, which is to be used for propelling or lifting are inclined at an angle near about 22° to the direction of motion, provided the blades be constructed of a rigid plane substance.

II.—If the blades be shaped as small sectors of a circle the point of maximum pressure at any velocity and inclination lies close to a point $\frac{5}{6}$ of the distance from the centre. From this point the pressure gradually declines towards the centre, but falls very rapidly towards the circumference.

Consequently the inclination of the outer sixth of the screw blade should be rapidly reduced from 22°, the amount of reduction depending upon the strength and nature of the material.

III.—The amount of surface of the screw blades should be about one-third of the area of the whole circle of revolution, and preferably less than more.

IV.—When screws are to be used for purposes of lifting or propelling, the best effect is produced when they are two-bladed, but when the available power is large and the screw is to be used for propulsion of air, or if the screw be actuated by a wind, a larger number of blades is an advantage.

V.—Other conditions being equal a plane, smooth surface is better than a perforated or imbricated surface, but in regard to concavity of the blade I am not prepared to offer an opinion.

VI.—When the blade of the screw is subject to "bulging" the angle of inclination should be proportionately lessened in order to allow for it.

VII.—The power required to raise and keep suspended an aërial screw is at the rate of 46 lbs. per horse-power.

VIII.—The difference in pressure above and below a revolving screw is potential, and at economical velocities there is no actual existing difference which is detectable.

IX.—The resistance offered by the air to a revolving plane inclined obliquely to the direction of its motion varies with the angle of inclination in such a manner that the velocities attained are in Harmonical Progression when the angles of inclination are in Arithmetical Progression and the relation is expressed by the formula

$$y = \frac{1}{ax + b}$$

where x is the angle of inclination.
y is the velocity attained (in revolutions);
a and b are Arbitrary Constants depending upon the power used to produce the motion of rotation

What is it, gentlemen, that stands in the way of a large scale endeavour to compass artificial flight by means of screws to lift and propel?

It is difficult to believe that it is the want of money, so small a sum as would suffice. The uncertainty of a return for outlay is the much more likely reason. The want of precise knowledge as to the dimensions, shape, pitch, and other qualities of the screw which will give the greatest effect for the least expenditure of power.

So long ago as 1842 it is reported that Mr. Phillips succeeded in raising into the air an apparatus weighing in the aggregate 2 lbs., by means of revolving fans with blades inclined at an angle of 20° from the horizontal.

I would ask you to notice this degree of inclination which was no doubt fortuitous, as it is only incidentally noted in the report.

"All being arranged the steam was up in a few seconds, when the whole apparatus spun around like a top and mounted into the air faster than any bird." This is the description of an eye-witness. It is stated that the apparatus afterwards fell and was broken, and there is no record of a repetition of the experiment.

I cannot conclude without an acknowledgment of help from others, without expressing my thanks to Mr. George Thompson, of Campbell College, Belfast, for his mathematical assistance, to Mr. I. Smith Bunney, who has at various times made for me the apparatus I have required, and to you, sir, and gentlemen, for so patiently listening to me.

TABLE OF VELOCITIES AND INCLINATIONS.

	Date.	0°	10°	20°	30°	40°	50°	60°	70°	80°	90°	
1	15 May, 1907	—	—	—	129	—	90	70	57	57	—	Revolutions per 10 seconds
2	14 ,, ,,	—	153	137	95	87	64	58	54	54	51	,,
3	15 ,, ,,	136	132	100	82	—	72	66	56	51	50	,,
4	14 ,, ,,	112	104	84	68	62	58	48	39	34	34	,,
5	26 ,, ,,	165	110	84	67	55	44	43	33	33	30	,,
6	30 ,, ,,	—	—	—	106	—	—	75	—	—	—	,,
7	30 ,, ,,	—	186	141	—	—	—	—	72	—	69	,,
8	May, 1906	167	120	89	—	56	—	43	—	33	30	,,
9	—	164	—	87	—	53	—	43	—	—	—	,,
10	—	160	—	86	6	—	—	—	—	—	—	,,
11	—	167	—	—	60	—	—	—	—	—	—	,,
12	15 May, 1906	172	120	74	54	48	35	33	33	32	29	,,
13	do.	—	125	77	58	47	40	33	33	32	29	,,
14	do.	142	110	81	59	51	43	38	—	—	—	,,
15	6 June, 1907	168	—	84	72	60	—	46	43	—	—	,,
16	do.	—	—	84	72	58	—	46	43	—	—	,,
17	17 June, 1907	—	—	121	80	59	—	37	32	29	—	,,
18	do.	—	—	134	—	84	72	64	53	46	—	,,
19	do.	210	142	135	92	84	72	64	53	42	40	,,
20	do.	35·35	26 71	21·36	17·89	14·68	11·74	11·48	8·81	8·81	8 27	feet per second at periphery
		35 35	26·18	20·46	16·90	14·39	12·54	10·82	9·96	9·04	8·29	Harmonical Progression

The CHAIRMAN: Would any gentleman like to ask any questions, or to make any remarks on this paper?

Colonel CAPPER: I should like to ask the lecturer whether he tested the area of the screws—the proportion of the area of the air blades to the total area of the whole screw circle; whether he also went into the question of the velocities. He gave us figures, I think, that showed that with area of blades 11 per cent. of that of whole circle you get a screw lifting itself, but the lift for a given power was greater when you had about 33 per cent.—I am not sure of the exact figures. I should like to know whether he also took the velocity of the screws—the larger area screw as compared with the smaller area screw, when each was lifting itself. I think that is a very interesting thing to note.

Mr. WOOSNAM: A point I should like to ask very much is with reference to the number of blades in a propeller. I take it that the maximum proficiency would be obtained if it were possible for each propeller blade to act upon virgin air. I should like to ask whether Mr. Parsons has taken into account the difference that the propeller blades would encounter when moving through the air, that is to say, were his experiments carried out with a propeller blade that was stationary? If the propeller is revolving and is at the same time moving forward, the air has a greater chance of being less disturbed, even if there were a greater number of blades. I do not know if I make my point clear. When a propeller is revolving at a high number of r.p.m. and is fixed in the same spot, although cavitation may not occur, yet there must be a tendency towards it, but if the propeller is also moving towards undisturbed air at a rate of 40 or 50 miles per hour, this tendency to cavitation must be reduced, for there is a greater inrush of virgin air to be handled by the blades. Therefore is it not possible that although a four or six bladed propeller is not so effective as a two-bladed one when stationary, yet the greater number of blades might become more effective as the velocity of the aërodrome increased.

Mr. CLARKE: The lecturer, in mentioning one formula, viz., that of Duchémin, gave the maximum lift as at 36 degrees. Should not this maximum be at 26 degrees? I happened to go into this matter a little while ago. I saw in one of Major Baden-Powell's papers that at a given speed the maximum lift for a plane was at 26 degrees. This also agreed with Langley's results. Working out the above formula it also gave its maximum lift at 26 degrees. This also, I think, works in very well with the lecturer's results, in which he said that between 22 and 30 degrees he got some of the best values of the inclination. In that case it looks rather

that the lecturer has given us a new formula of the relations between the velocity and pressures which seems to agree for propellers even better than that of Duchémin.

Mr. PARSONS: In response to Colonel Capper, I may say I did measure the difference. I took the area of the screw blades in regard to the whole circle of revolution, and 0.3 refers to that proportion; that is to say, that the most effective area to use is such as would occupy one-third of the whole circle of revolution, not merely of available blade area. In regard to the velocity of this larger-area screw, I have had to pass it over rapidly, but I intended to say that I got a velocity of about 13 revolutions per second at 22° of inclination, which, reduced to angular velocity, brings it to 29.30 feet per second, equal to a pressure of about 1.901853 lbs. per square foot.

80 revolutions in $\frac{20{\cdot}5}{200}$ minute (metronome) = 13.01 per second
= 29.30 ft. per sec., angular velocity at centre of pressure.
Pressure = 1.90 lbs. per sq. foot (Wolff's table)

on 0.6498 sq. ft., the area of the surfaces of my blades.

In response to Mr. Woosnam, Professor Langley makes a great point of that (propeller blades striking virgin air). He speaks of the plane encountering new fields of air which have not had their inertia disturbed.

A good deal is also made of that point by Mr. Lanchester in his theory of peripteral zones, but I have not had time to go into it, as this book has been so recently issued from the press. My experiments were made with the propeller fixed, except in so far as it was able to rise to the extent of three or four inches. (It was held down by safety catches, except in those experiments of throwing it up to the ceiling. I did not go on with them because they did not lead to any definite result.) In regard to the others it was a question of finding out the least power which could lift a given weight, of finding out the best area, of loading up the propeller to the greatest weight which a particular power was capable of lifting, and of inclining the planes at that angle which would cause the screw to be lifted up with the least power and greatest weight.

In answer to Mr. Clarke, I may say that I am not responsible for the statement that 36° is the angle of maximum efficiency in the Duchémin formula. It may be an error of transcription. I have not estimated it. On the other hand, the other formula that I gave you, that of De Louvrié, is stated to agree very well with this emperical formula, and it is stated to attain its maximum at 39°. I do not feel inclined to say it is not 26°, especially if the gentleman is speaking from the Smithsonian publication of Langley's work. In the book from which I have copied there are several misprints which I have noticed at one time and another. However, I am not prepared to contradict Mr. Clarke, sir.

The CHAIRMAN: I am sure that we have all been interested in this very valuable and technical paper. This subject of screw propellers and the really scientific explanation of their action is one which we badly need to have carefully considered and discussed, and I am sure that this paper is a most valuable contribution to our knowledge of the subject. I must make one remark after what Mr. Clarke said about my little experiments because in those trials I rather think—I am only speaking from memory—but I think I made 15 degrees as the most efficient pitch for lifting propellers. But at the same time I do not wish to lay any stress on that, because my trials were not very accurately carried out, and the means I had at my disposal were not sufficient to make very accurate tests, so that when I put 15 degrees down as being the most efficient angle for a propeller I couldn't say within a good many degrees whether it was really best or not—only somewhere about that. There is one other remark I should like to make about the number of blades in a screw propeller to lift. I rather think I showed in this very hall some years ago a little propeller that I made with only one blade. It seemed rather absurd to have only one blade to a propeller, but this little instrument with twisted india-rubber was able to rise to the ceiling, showing the efficiency of the arrangement. Of course, having only one blade it was badly balanced, and I have no doubt that if constructed on a large scale it would give an uneven and wobbling effect—but it was an interesting result. Well, I won't detain you any longer, but I am sure you will accord Mr. Parsons a vote of thanks for the very able and interesting paper he has given us to-night. (Applause.)

The CHAIRMAN: I will now ask Mr. José Weiss to read his paper on the "Aspects of Sailing Flight."

Mr. JOSE WEISS read the paper.

Aspects of Sailing Flight.

By Jose Weiss.

In my paper about "Starting Methods," which appeared in the last issue of the "Aëronautical Journal," I pointed to the fact that one of the reasons why the aëroplane of the future is not likely to start unassisted from the ground is that the power required for starting is much in excess of that required for actual flight, and that, consequently, if we have to carry on board an engine strong enough to raise our machine by running it on the level ground, that machine has to be much stronger, heavier, and larger than is necessary for carrying any specified weight. We fall into a vicious circle. The high figures of the Farman aëroplane are a good illustration of this fact. To carry one single man we have here a machine reaching a total weight of 1,200 lbs. and a 50-h.p. engine.

The reason of this lies in the general principle of sailing or gliding flight, which, in a nutshell, is about this: In the sailing bird or in a properly made glider, the main portion of the motive power is derived from the weight, the trajectory produced by the weight in following the path of least resistance depending entirely on the perfection of the aggregate features of the bird or glider. If we assume, for instance, in a perfectly balanced glider, the resistance to horizontal penetration to be equal to O, and the lift, that is the resistance to vertical fall, to increase with the speed, the result must be perpetual motion in the horizontal direction. Now, these are the very conditions which obtain in the sailing bird; we have a form which offers no appreciable resistance to penetration; we have the lift increasing with the speed; and we have also the resulting virtual perpetual motion which we observe daily.

I have often seen my own roughly-made motorless gliders, when at their best, reach in calm air 3° and even 2° from the horizontal, and in ascending currents they frequently rise to a considerable height whilst travelling against, or circling in, the wind, exactly in the same manner as a bird. A snapshot, which you will see presently on the screen, shows one of these successful glides. What wonder, then, that the sailing bird, in which every feature is absolutely perfect, and which has a living balance, should reach, in "falling," a line of flight which is practically the horizontal. It is obvious that the power required to reach the horizontal absolutely becomes, in this case, infinitesimal. In nature, such additional power as may be wanted is easily supplied by the least ascending currents, of which the bird takes every advantage, and so it is that it sails indefinitely, without expending the least power, as the mere result of gravitation. The supposed mystery of the beautiful ascending orbs of the larger sailing birds is thus fully explained. Whenever these ascending orbs are observed it is on a more or less windy day, and invariably on the wind side of the hills, or above a group of trees or other large object capable of producing an ascending current. Never, during years of observation, have I witnessed these orbs over the lee side of the hills, and unless the wind and the nature of the ground below were such as to account for the existence of ascending trends. If, then, the falling rate of the bird or glider be less than the ascending rate of the wind, the result must be that apparently paradoxical, but, nevertheless, very natural, fact that the bird or glider is falling while actually rising; that is to say, it is falling in relation to the supporting and ascending air, but is rising in relation to the earth below.

Once we thoroughly understand this, we can well realise why it is that the larger birds have such difficulty in starting flight, and why also a one-man aëroplane, weighing 1,200 lbs., requires no less than 50-h.p. to start unassisted from the level ground. It is because the weight does not come into play as motive power until the bird or aëroplane is actually on the wing.

If we take as a base the "Issy" experiments we can say that, roughly speaknig, the power required to raise an aëroplane from the level ground is a thrust equal to about one-third of the total weight of the machine.

Whether the machine be of a good type or not would not materially change this proportion of thrust to weight for a start from the ground. Once, however, the machine is on the wing, the thrust required to keep it afloat depends no longer on the weight of the machine, but on its quality as a glider; it depends on the nearness to the horizontal of the line of flight which the machine gives by its own weight, unaided by power.

I believe myself, from results obtained with heavy models, that a pedal-driven

machine, requiring no more effort than an ordinary bicycle, is perfectly feasible, on condition that suitable launching ways are available for a start. It can be done if the total weight of the machine, including the rider, does not go much above 250 lbs., and if that machine glides by its own weight within no more than 4° from the horizontal. For such a machine an average thrust of about 20 lbs. is ample, and this a man trained to the bicycle can easily develop with a suitable aërial propeller. Here man has the advantage over bird. The latter has to take things as it finds them. We men can make things to suit our requirements, that is, we can make launching ways such as those which I will show you presently on the screen, and which enable us to put on the wing a light and inexpensive machine unhampered by and not depending on a powerful motor carried on board.

One of the slides to be shown is a table of sailing birds. The main point of this table is the relation of weight to wing area. That a correct relation between weight and wing area is an important factor in sailing flight is very obvious from the fact that, since a glider can, from experience, be underloaded or overloaded, one particular intermediate relation of weight to surface must necessarily be the optima relation. You will observe from the table that in nature that relation is not one of direct proportion, the constant being

$$\text{not } \frac{\text{weight}}{\text{area}} \text{ but } \frac{\text{weight}}{\text{area}^{\frac{4}{3}}}$$

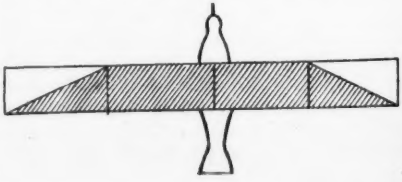
In all birds capable of real sailing the value of that $\frac{W}{a^{\frac{4}{3}}}$ remains at 1.6 or thereabouts. When below 1.6, as in the case of the peewit and of the heron, or above 1.6, as in the case of all fast flapping varieties, the bird is no longer a real sailer.

This seems to point to the existence of a law so far unknown. On the other hand, experiments with model gliders of graduated sizes show peremptorily that the relation is exactly the same in artificial gliders as it is in birds; that is to say, that two gliders of exactly the same type, but of different sizes, will glide at the same speed if, and only if, loaded in relation to the power $\frac{4}{3}$ of their surfaces. How this can be made to tally with the old classical formula KSv^2, which all mathematicians place at the base of their calculations, I cannot say. Huxley's famous saying, that what comes out of the mathematical mill depends on what is put into it, befits this case admirably. The data available at the present time are inadequate to arrive at anything definite from mathematical deductions, and we have, therefore, no option but to resort to empirical methods. There are at least three factors which, as far as I know, no mathematician has ever brought to bear on his equations. The first is the all-important action of the weight as motive power. The second is the beautiful horizontal component produced, under the action of the weight, by a rigid and bent-down front edge and a very flexible rear edge. The third and most important is the powerful vertical suction produced by a current of air striking a convex surface tangentially, and which, so far, is not only undefined, but completely ignored. I had hoped to have something more to say to-day about this mysterious reaction, but the experiments which Mr. H. Page and myself are carrying out, with a view to define its properties, are as yet incomplete, and the paper relating to them has to be left for a future occasion.

TABLE OF SAILING BIRDS.

Name.	Wing area in sq. feet.	Weight in lbs.	Lbs. per sq foot.	Weight / Area $\frac{4}{3}$.
Swallow	0·154	0·045	0·29	1·4
Black Swift	0·198	0·10	0·45	1·9
Tern	0·423	0·25	0·58	1·7
Small Gull	0·984	0·69	0·70	1·6
Another Gull	1·914	1·68	0·88	1·6
Another Gull	1·232	0·95	0·78	1·6
Black backed Gull *	3·834	5·00	1·15	1·6
Hawk	0·866	0·44	0·51	1·3
Rook	1·778	1·08	0·74	1·5
Albatross *	10·164	17·00	1·67	1·7
Vulture Gyp Fulvus *	11·440	17·50	1·53	1·6
Man-carrying machine, based on above	86		3·20	1·6

The table is compiled from personal observation except in the case of the birds marked*, when the information was obtained from the Natural History Museum. All the birds were measured by the following method for purpose of comparison:—



The wing area (shaded part) is deemed equal to three-quarters the span × greatest width, the span being taken when the rear of the wing makes a straight line, and no count being taken of either head or tail.

The CHAIRMAN: Has any gentleman any questions to ask or any remarks to make? If no one has anything to say, I think I cannot but ask you to accord your vote of thanks to Mr. Weiss, who has given us a very interesting paper. I am sure that we all know of his experiments, and how very interesting they are, with these gliding models that seem to act so very satisfactorily, and it is very interesting to hear about the launching ways that he has erected. I am sure some of us will be glad to see them. It rather reminds me of a certain structure that I put up once for launching gliding machines, and which I also found was hardly big enough. My appliance was just 50 feet long—a little bit longer than Mr. Weiss's, and I found that when we put a full-size machine on to it to run down on little wheels, that it did not get up enough speed to really do much good. We did get some glides, because in that case the machines were run off at the end of the track, which was at a considerable height, about 8 or 10 feet, above a sheet of water. But I expect it will be found necessary to make something a little larger to act satisfactorily. There is no objection to the general principle of making such a structure, and it is a specially good idea to make it so that it can revolve and face the wind in any desired direction. I can only ask you to accord your vote of thanks to Mr. Weiss. (Applause.)

Major BADEN-POWELL: I shall now crave your indulgence for a few moments to explain to you some little models that I have here. Major Baden-Powell then read his paper on "Experiments with Dipping Planes."

Experiments with "Dipping" Planes.

By MAJOR BADEN-POWELL.

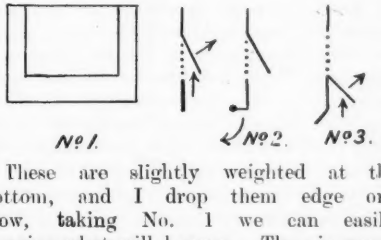
It has been usual, in considering the theory of aëroplanes, to assume a plane or combination of planes so fixed that they present an inclined under surface to be acted upon by the air. Yet it is generally found in practice that such surfaces do not satisfactorily fulfil their object. Curved or arched surfaces seem to do better, and several experimenters have gone further and arranged these aërocurves in such a manner that they do not present any under surface to the impinging air. Hargrave tried a curved surface the cord of which was set at a negative angle to the front. He found that an eddying current was set up in the concavity helping to sustain and propel the apparatus. Phillips arranged sustainers with a curved upper surface, but with a flat and horizontal under surface. He concluded that the air was driven upwards off the curve, forming a partial vacuum over the after part. But Langley, on the other hand, made out that a plane propelled horizontally without any inclination did not sink in the air.

I have found that if the front edge of a plane be inclined downwards it may still derive support from the air against which it is driven.

It is not my intention to-night to describe what I consider to be the best form for a practical machine, but merely to put forward a problem in aërodynamics which calls for explanation.

I will now show some simple little paper models.

Here are several sheets of stiff paper each having panels cut round three sides and bent back; thus—



These are slightly weighted at the bottom, and I drop them edge on. Now, taking No. 1 we can easily imagine what will happen. The air pressing normally against the under side of the flap will drive the whole away towards that side; it will also cause the whole to tilt so that the flap is upwards. But now on dropping it, you see that that is just what does *not* happen! It turns the other way with the flap downwards. This result may be explained by the weight of the flap, making the centre of gravity lie towards the side of the flap. We will, therefore, place a clip sticking out towards the other side, and, dropping this, we see it still turns with the flap downwards. It has been suggested that this is due to a rudder-like

action, the plane turning about its centre of gravity. So we will try the flap the other way, as No. 3. Now the action of the air in the "pocket" formed must act strongly on the flap and turn it. But it doesn't. Some think the action due to the border, so I have made up a couple of planes attached by a stick, but still they act the same.

The dropping of these planes vertically should be exactly equivalent to propelling them horizontally, except that gravity does not bias their action. If so, when propelled along with planes pointing downwards, the tendency would be for them to rise against gravity. I, therefore, produce a piece of apparatus consisting of a series of inclined planes one behind the other. If this be thrown dart-like when the planes are inclined upwards it goes but a short distance and generally tumbles over, but when turned upside down so that the planes "dip" or incline downwards in front you will see it glides very nicely.

Though I have been thinking a good deal over this matter, I have not arrived at any entirely satisfactory solution of the action. It may be that each result is due to some different cause, but I hope some of you may be able to offer some explanation.

There is one other little experiment that I can show which might throw some light on the matter. Here I have a plain piece of cardboard with a number of little tabs of tissue paper gummed on to it at one end, and, of course, these show the direction in which the current is blowing. Now what I want to show you—I don't know to what extent you can see it—is that when held nearly edge on to the blast from the electric fan, so that the current is running along the surface of the plane, of course the little tabs are blown straight back. As the angle of inclination increases you will notice that a good many of those little tabs on the back of the plane are facing the opposite direction. That proves that the air circles round on the back side of the plane, and is going in the opposite direction to what it is on the other side. Some very careful experiments might well be made with this in order to ascertain exactly the angle at which the current begins to go backward. I have not been able so far to get at it exactly, because there is a lot of eddying wind so that the tabs flap about a good deal before they take any really decided forward action. (Applause.)

Mr. Senecal: I should like to ask the lecturer whether he took a very great distance between the two planes? The distance, I should say, was twice the width of the planes.

Mr. Weiss: I think I can give a simple explanation of the behaviour of these models when dropped to act under their weight. Whatever they do they follow the path of least resistance. The direction in which the flap offers the least resistance is edgewise, and it falls edgewise the whole time. There would be resistance if it was to go in any other way. In the same way in the double model it must go better if the planes are pointing downwards, because that is the direction in which it tends to fall. You must bear in mind that as soon as the thing begins to glide, the centre of pressure moves forward and raises the head. There is no mistake that this is the principle of sailing flight. I do not know whether I told you before about some experiments I made three or four years ago with a dead bird. I fixed the bird in the attitude of flight in an open cage made of battens, and inside the bird was fixed in such a way that it could not move its position, although it could go backwards and forwards about a couple of inches. Whenever this was exposed to strong wind the wind had no effect on it, the resistance could not be appreciated; in fact, a gust of wind seemed to throw the bird forward. I know well from experience with my own models that whatever the strength of the wind, if they are held exactly with their nose to the wind one cannot detect the least pressure on the model.

Mr. Reid: If I may be allowed I should like to repeat a remark of Mr. Weiss about the mathematical mill. I am glad to find that you have not put that mathematical mill into operation, for although one does not wish to depreciate in any way the value of mathematics, yet if there is time wasted in connection with aëronautics it is over the mathematical part of it. There are few accurate data, and they are made the foundation for a great deal of figuring. I think you have just now given us a very good object-lesson indeed; that things which are supposed to be well known and data upon which most of our aëroplanes are being built at present are not all what they seem. It shows us the very great importance indeed of actual practical experiment. I did not say anything after Mr. Weiss's paper, but I did feel that he was advancing

our knowledge of aëronautics to a very great extent by these launching experiments. It is a defect we shall meet with, and there is only one form of aëroplane that can be raised direct from the ground, and that is one with lifting fans, but whether that will be practical or not we do not yet know. I thank you very much personally as we shall all do collectively directly for showing us these very ingenious experiments you have shown us. I have seen a great many others that you have carried out also, but I think this is one of the most instructive I have ever seen. (Applause.)

Mr. SENECAL: About 32 years ago in this very hall I made experiments with similar models; also with aëroplanes of various forms and sizes, such as square, round, triangular, etc., both in aërial lines and in rotation, and as these planes falling under the action of gravity, I found that their own weight and resistance was a sufficient explanation of their seemingly paradoxical motions, so long as they are perfectly flat, of the same thickness and material, and gravity being the only acting force. (The explanation will be found in the Aëronautical Society's Report of about 1875.) If an additional force is used in the form of weights or a series of weights, the planes will travel faster and their motions will be altered under the compelling direction of the weight. That is, the position of the weight on the plane, figuratively speaking, will compel the plane to follow the weight. I noted the following law, that the position of the weight or series of weights dominates and determines the direction of the translation of bodies, or of planes, or systems of planes, etc., however complex and whatever may be their relative forms. A plane will travel forward if the weight is placed in advance of the centre of gravity of the plane; the plane will travel faster as the weight slides away from the centre of gravity and pressure, so that theoretically the weight should slide forward continually. In practise this cannot be done. The weight can only be moved within the limits of speed and margin of safety. The weights that keep the vertical balance should be raised more in the line or axis of motion as the rate of speed increases. That is one of the true theories of balancing. When you start a machine have your weight low and raise it upwards and forwards as the rate of translation increases, and do the reverse as the rate of translation decreases. By practise you will control your machine in an effective manner. Active surfaces can be used with nearly the same results. I find in the triangular planes that the faster they are rotating the weight drifts more to the base of the plane, and as the velocity and rotation decreases the weight flies backwards. The law of centrifugal force explains the behaviour of the triangular planes. There was also a folded edge mentioned. A folded edge increases the weight as well as the resistance. There is also a decrease of pressure behind the folded edge. The explanation of flight of all flying beings, models, planes, etc., is found in the true co-relation of power, weight, and resistance. This explanation is quite sufficient for all practical purposes.

Major BADEN-POWELL: I may say that as regards that I have not tried them wider apart than you see them there, but I have tried them very much closer together. I made one which had a whole series of these planes close together, about four actually overlapping a little bit, and that went very well, but, unfortunately, I broke the stick of it, so I have not brought it here to-night. But I do not think the distance apart has much to do with the results. I think what Mr. Weiss said is very true. When the appliance begins to fall it has its edge on so that there is no resistance and, therefore, it gathers speed. But there is no doubt about it, as you can see—you can try them again later on—that these planes have a wonderful way of recovering elevation. If you give them a launch they sometimes give a little dip down, but they always rise again and seem to go so very stably that it seems to me it requires a little more explanation than merely to say that the plane follows the line of least resistance.

Mr. SENECAL: I may say I made experiments on that principle, and I found that an aëroplane piled one way would come forward; bodies with bristles, feathers, etc., are affective examples of the same principle—a sheet of velvet with a pile laid flat will come forward, but it will travel the other way only if you place a weight on the opposite edge. I may say that I will undertake to make any machine travel either backwards, forwards, sideways, or any other way, simply by altering the position of the weight.

NOTE.

[N.B.—With reference to Mr. Weiss' suggestions, I have since tried a device in which

the area of the border is far greater than that of the panel. One would then expect that the former would offer greater resistance to the air, but still we get the same result as before, indeed, the "dipping" action is more pronounced.—B. B-P.]

The CHAIRMAN: I will now ask Colonel Fullerton to read his paper.

The Farman Flying Machine.

By COLONEL FULLERTON.

So much interest has been aroused by Mr. Farman's successful flights that an account of his machine may, perhaps, be found useful. It is difficult to get accurate details of the apparatus, but it is hoped that the description given below may be found of some assistance to those who wish to design similar machines.

I.—GENERAL DESCRIPTION.

Speaking generally, the machine consisted of the following parts:—

(*a*) A double-decked aërosurface; the main sustaining surface.

(*b*) A body for carrying passenger, motor, etc.

(*c*) Small balancing planes, in front of the main aërosurface, for steering in the vertical plane.

(*d*) A box-shaped rudder, in rear of the main aërosurface, for steering in the horizontal plane.

(*e*) Motor.

(*f*) A propeller fixed in rear of the motor, working like the propeller of a marine ship.

(*g*) A chassis fitted with wheels, to facilitate starting and alighting.

(*h*) Seat for passenger.

As regards the general construction of the machine, etc.:—

(1) The framework of body, sustainers, etc., was made of steel tubing, the covering of aërosurfaces, etc., being of canvas.

(2) The total weight, in order of march, was 530 kilog. (1,168 lbs.).

(3) The total sustaining surface was 52 sq. m. = 560 sq. ft. The area of the balancer (*c*) is not counted as a sustaining surface.

(4) The sustaining velocity does not seem to have been accurately measured, but apparently the machine moved on a level course, when its speed was from 14m. (45.9 ft.) to 17m. (55.8 ft.) per sec., the inclination of the sustaining surfaces being about 12° in the former case and about 6½° in the latter.

(5) The main aërosurface consisted of two superposed surfaces each 10m. (32.8 ft.) wide by 2m. (6.56 ft.) in length, the total surface being equal to 40 sq. m. (430.5 sq. ft.). The surfaces were arched, the arching being about $\frac{1}{14}$. When the machine was in full flight the angle of inclination of the chord of the arch varied between 12° and 6½°. The vertical distance between the two surfaces was 1.5m. (4.92 ft.), and the connecting posts were stayed and kept in position by suitable wire guys.

The surfaces were fixed at a dihedral angle of about 140°.

(6) The body consisted of a tubular framework covered with canvas, fair-shaped in front, but with a vertical stern. The extreme width (transverse direction) was .75m. (2.46 ft.); length 4m. (13.12 ft.). The passenger's seat was in such a position that his centre of gravity when seated was in the vertical line passing through a point .25m. (.81 ft.) in rear of the front edge of the main aërosurface.

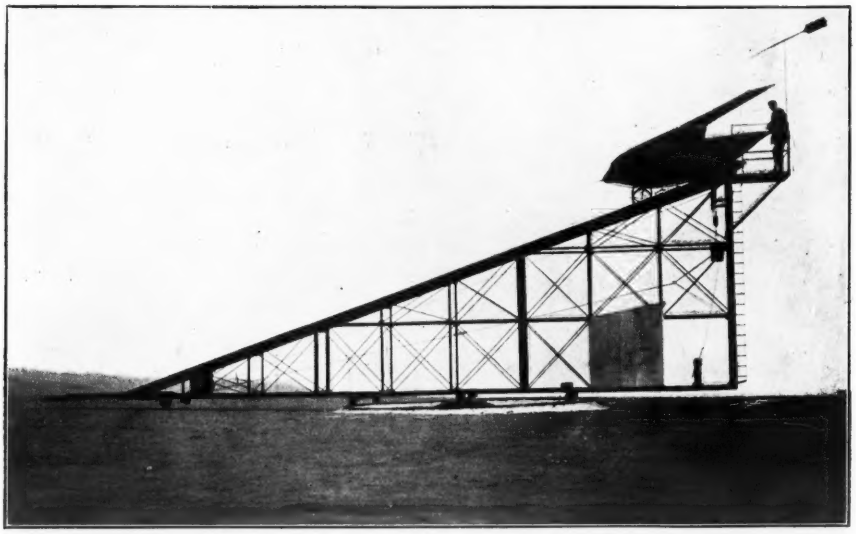
(7) The balancing planes consisted of two wooden surfaces each 2m. (6.56 ft.) by 1m. (3.28 ft.). In section the planes were arched above, but had very nearly flat undersurfaces. They swung on a pivot fixed at .25m (.81 ft.) from their front edges, and could be moved up and down by a suitable gearing worked by the passenger.

(8) The box-shaped rudder was 2m. (6.56 ft.) wide, and 3m. (9.84 ft.) long. The upper and lower surfaces were arched $\frac{1}{14}$, and the angle of inclination of these surfaces in full flight was about 12°. The stays, guys, etc., were similar to those used on the main aërosurfaces. The area of the upper and lower surfaces, which both helped to sustain the machine, was 12 sq. m. (129.17 sq. ft.). The front edge of the box was 6m. (19.68 ft.) in rear of the front edge of the main aërosurfaces.

(9) The motor was an 8-cylinder petrol Antoinette, giving 50 Fr. H.P. as a maximum. Its weight was 80 kilog. (176 lbs.); there were no cooling appliances. The centre of gravity of the motor was in the vertical line, passing through a point about .7m. (2.3 ft.) in advance of the rear edge of the main aërosurfaces.

Fig. 1.

THE WEISS LAUNCHING WAYS.



For description see the "Aeronautical Journal" for January, 1908.

Fig. 2.

THE WEISS 28 lbs. MODEL (Soaring).

Surface = 18 sq. ft. Net Weight = 12 lbs. Ballast = 16 lbs. Total Weight = 28 lbs.
The Model is rising on the ascending current, the direction of which is shown by the smoke of the bonfire below.

Fig. 3.

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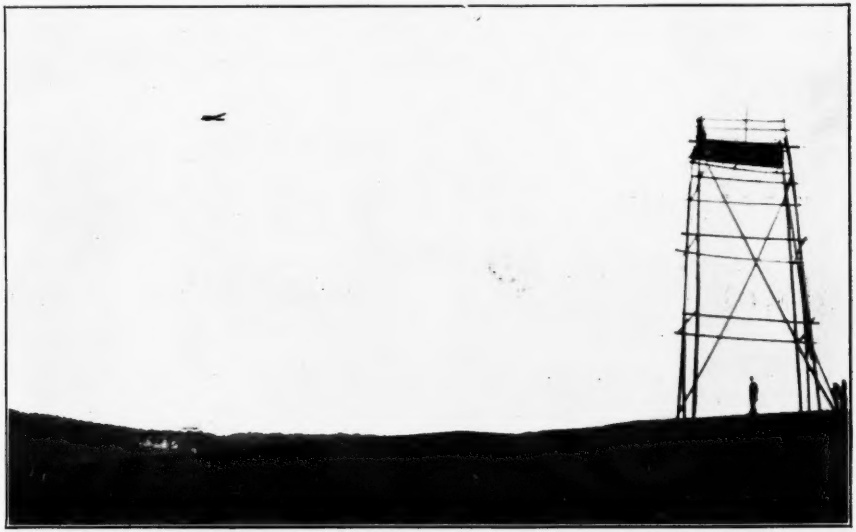


Same Model as in Fig. No. 2; it is flying right away from the Camera, and is seen as a mere speck.

(10) Propeller. This was of the driving type used in marine ships; it had a steel frame and was covered with canvas. The diameter was 2.3m. (7.54 ft.); pitch 1.4m. (4.6 ft.). It was mounted directly on the motor shaft, and at 1,050 revolutions per minute required 38 Fr. H.P. to drive it.

(11) The chassis was constructed of steel tubing. The wheels were of the usual bicycle type, the front one being .25m. (.81 ft.) in diameter, the rear ones .13m. (.425 ft.).

(12) Starting. To start, the machine was simply run along the ground under its own power until it rose in the air, movements in a vertical plane being controlled by the balancing planes.

(13) Alighting. To alight, the power was shut off, and the machine glided down to the ground on a gentle slope.

II.—TRIALS.

The trials of this machine were carried out in a very methodical manner, Mr. Farman's great object being to get a thorough knowledge of the working of the apparatus before attempting to leave the ground.

The earlier experiments were made in September, 1907, and were not very successful, as the best flights only reached 30 or 40 yards in length. Improvements were, however, made from time to time, and the distances increased to 100 yards, 120 yards, etc., until rather unexpectedly on October 26th, 1907, a flight of 770 yards was accomplished. Circular flights were next tried, and at first great difficulty was experienced in taking a curved path, as the machine always dropped to a lower level when the rudder was put over.

On January 11th, 1908, however, a circuit including two curved paths was made in 1 min. 45 secs., the total distance traversed being 1,800m. (about 2,000 yards).

Mr. Farman then decided to try for the Deutsch-Archdeacon prize, and on January 13th, 1908, succeeded in winning it.

He is reported to have covered a distance of 1,500m., including a curve of 220m. radius, in 1 min. 28 secs. This would make the velocity about 17m. per sec., but as the inclination of the sustainers was apparently from 10° to 12°, it is doubtful whether the real velocity ever exceeded 14m. per sec.

On January 14th, 1908, some further trials were made, chiefly to test the amount of extra weight which could be carried. (Boxes of water were used to represent these weights.) In the first trial without extra load, 1,500m. was covered in 1 min. 33 secs. (one turn); subsequent trials gave the following results.

Load of 15 kilg. ...	400m.	(one turn)
,, 20 kilg. ...	100m.	(straight)
,, 25 kilg. ...	30m.	(straight)
,, 30 kilg. ...	*nil*	

The H.P. used has not been recorded, and no details have been published as to the position of the extra weights.

III.—REMARKS.

Before criticising this machine it is necessary to clearly understand that it was constructed for a special purpose, viz., to cover a distance of at least one kilometre on a circular course. To this end everything was subordinated, and the question of providing a machine capable of carrying more than a very limited amount of effective load (viz., passengers and stores) was not considered.

This should be recollected when examining the design, and full allowance must be made for the peculiar conditions under which the trial was carried out.

(1) *General Type.*—Mr. Farman selected this type of machine chiefly because of its stability, but also on account of the simplicity of its constructional details. On the whole it worked well, and would seem to be a good class of machine for elementary attempts at flight.

(2) *The great weight and size* of the machine is very noticeable, especially when it is remembered that it only carried one man for a very short distance. A weight of half a ton and a space occupied of 1,200 sq. ft. appear to be much too great for such a small effective load.

(3) *Power Expended.*—This also was very large, no less than 38 Fr. H.P. being used.

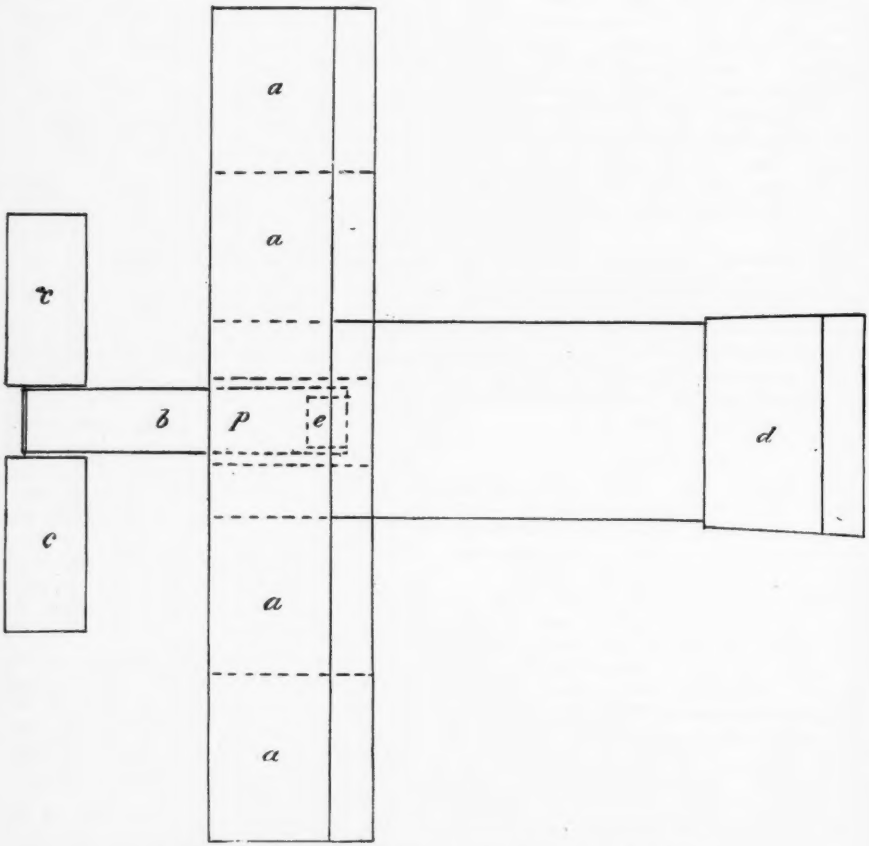
The lift was

$$\frac{530 \text{ Kilg.}}{38 \text{ H.P.}} = 14 \text{ kilg. per Fr. H.P.},$$

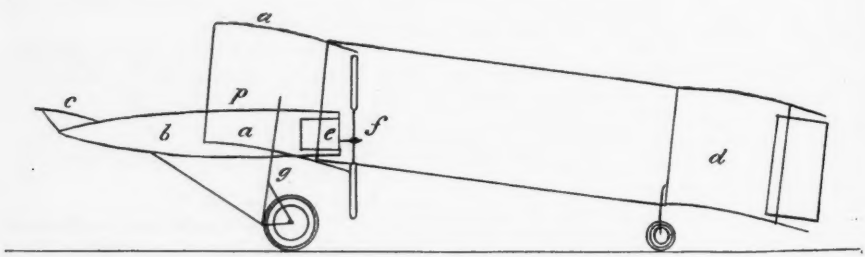
or about 30.8 per B.H.P.

(4) *There are not* sufficient details to calculate the thrust from the propeller dimensions, but the actual thrust was at 16.67m. per sec.

$$\frac{38 \times 75}{16\,67} = 170.6 \text{ kilg.};$$



The Farman Flying Machine. Fig. 4.—Plan. Scale 1/80.
(By special permission of the Motor Supply Co., 111, Piccadilly, W.)



The Farman Flying Machine. Fig. 5.—Section on Centre. Scale 1/80.
(By special permission of the Motor Supply Co., 111, Piccadilly, W.)

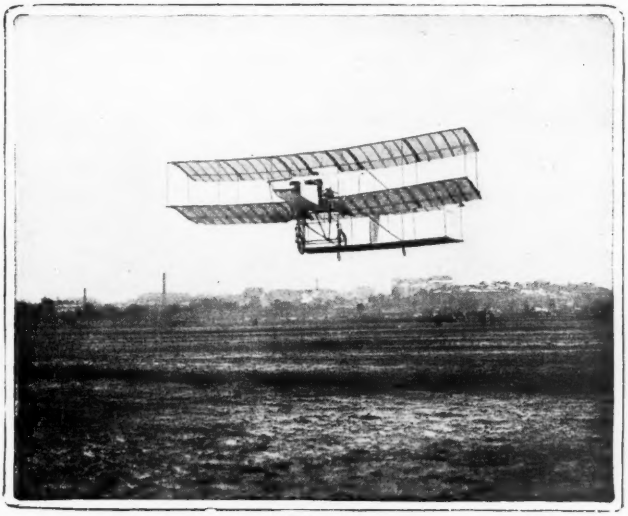
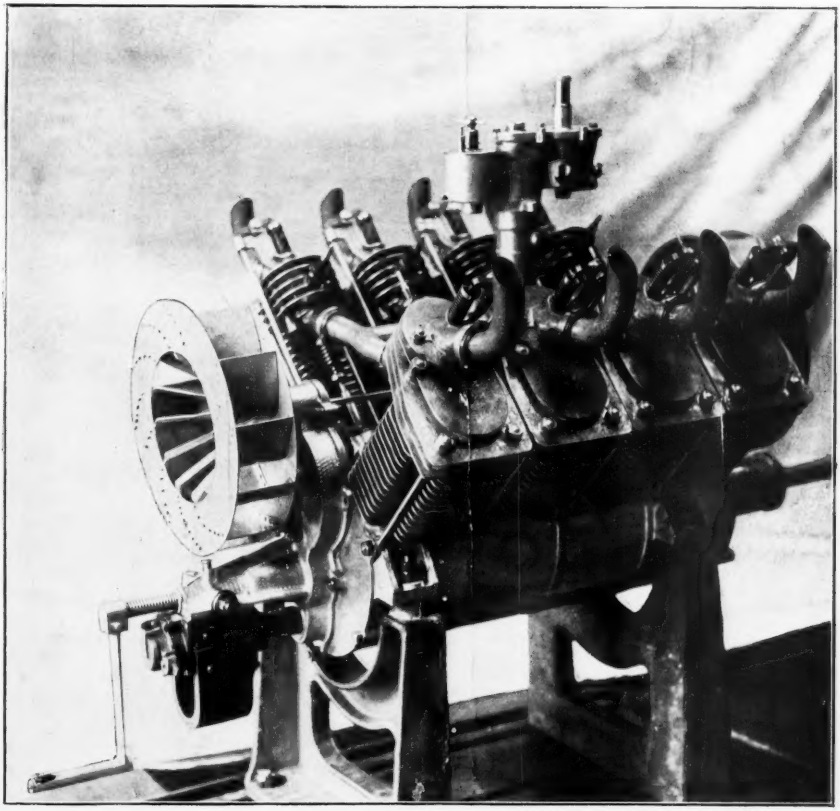


Fig. 6.

THE FARMAN FLYING MACHINE.

By special permission of the Motor Supply Company, 111, Piccadilly, W.



By special permission of the] **Fig. 7.** *[Motor Supply Company, 111, Piccadilly, W.*

THE FARMAN FLYING MACHINE.

hence

$$\frac{\text{drift}}{\text{lift}} = \frac{170{\cdot}6}{530} = \frac{1}{3{\cdot}1}$$

At 14m. per sec.

$$\frac{38 \times 75}{14} = 204 \text{ kilg.};$$

hence

$$\frac{\text{drift}}{\text{lift}} = \frac{204}{530} = \frac{1}{2{\cdot}6}$$

which results do not compare very favourably with the Maxim or Phillips machines.

(5) *Calculation of Lift, etc.*—Owing to the doubt as to the real speed, it is difficult to check the results with well-known formulæ, but it is worth while noticing that the lift and drift check fairly well with the diagrams given in Table IV. of "Der Vogelflug" by the late Herr Lilienthal. Thus with V = 14m. and $\alpha = 12°$

Lift $= .612 \times .09 \times 52 \times 14^2 \times \cos 12°$
$= 549.6$ kilg.

Drift $= .612 \times .09 \times 52 \times 14^2 \times \sin 12°$
$= 116.412$ kilg.

Since the total thrust at this velocity was
$= 204$ kilg.,
the body resistance was
$= 87.6$ kilg.,
a very considerable amount.

Similarly, when V = 16.67 $\alpha = 6\frac{1}{2}°$, the body resistance was
$= 109.5$ kilg.

It is clear from the above calculations that the large amount of power required to drive the machine was due to the body resistance, which, on the average, was equal to the resistance of the sustaining surfaces. No doubt this will be altered in future machines.

(6) *The Body.*—It is not quite clear why the square-shaped stern was adopted. This shape adds considerably to the forward resistance, and a fair-shaped body might be used with advantage.

(7) *The Balancing Planes* seem to have worked very well. Their shape is noticeable, as the section is very similar to that of one of the sections used by Mr. Horatio Phillips in his earlier experiments.

(8) Owing to the conditions of the trials the design of the rudder was of special importance. It seems to have acted well, but it is doubtful whether such a large surface is desirable. Again the great distance between the rudder and the main aërosurfaces was decidedly disadvantageous from the constructional point of view.

(9) *The Motor.*—This was of the Antoinette type, and worked well. The absence of cooling arrangements, however, made a long flight out of the question.

(10) *The Propeller.*—Very little information is available about the propeller. At 16.67m. velocity the slip

$$\frac{16{\cdot}67}{1{\cdot}40 \times 17{\cdot}5} = .68,$$

and slip = 32 per cent.

At 14m. velocity
the slip = 43 per cent.,
on the supposition that the revolutions were 17.5 per sec. in both cases.

(11) *The arrangements* for starting and alighting worked very satisfactorily, and there does not seem to have been any trouble in executing either of these manœuvres.

(12) *As regards the trials*, they seem to have been well and carefully carried out, special attention having been paid to familiarising the passenger with the working of the machine, *before leaving the ground*, an important detail which should be copied by all aëronauts when testing their apparatus.

NOTE.

Since the above was written Mr. Farman has made a number of very successful flights, notably one on March 21st, 1908, in which he covered about 3½ kilm. in 3 mins. 47 secs. (Velocity = 51 kilm. or 31.7 miles per hour approximately.)

Members of the Society will be glad to hear that his accident on March 27th was not so serious as at first reported. The latest accounts state that he is doing well and hopes soon to go on with his experiments.

The CHAIRMAN: Does any gentleman wish to make any remarks?

Mr. WEISS: I should like to ask what was the actual thrust of the Farman motor when tried. I have not been able to get the record.

Colonel FULLERTON: The details of the propeller are not sufficiently well known to work out the thrust. Approximately from other information I make it 170 kilg. at 16.67m. per sec., and 204 kilg. at 14m. per sec.

A GENTLEMAN: Could you tell us the position of the propeller?

Colonel FULLERTON: It was fixed upon the motor shaft immediately in rear of the boat-shaped body.

A GENTLEMAN: I understand that at one time there was a helm or rudder at the back of the machine. I do not know whether this was the case in his last flight.

Colonel FULLERTON: Yes, that box at the back is the rudder.

A GENTLEMAN: There was not an actual rudder to move.

Colonel FULLERTON: He could move the box at the back a little, as far as I can make out, but I cannot give you the details.

Mr. SPENCER: Would you allow me to correct you? The box at the back has a rudder about half its length inside. You are incorrect in saying that the whole box moves; it does not, it is fixed absolutely at right angles. The rudder itself is moved by cords held by Mr. Farman.

Major BADEN-POWELL: When I first heard of this mishap to Mr. Farman to-night I thought he was probably trying his new machine, because I cannot help thinking—I daresay it has occurred to many of you, too; I know it has to those I have spoken to—that from the published designs it certainly does not look to me very serviceable. I suppose he knows better than we do what it is likely to do, but if that new machine succeeds, and if it is made as the public designs show, it will certainly give us some rather curious and unexpected effects, as the whole balance of the machine is so different from what has been tried before. I ask you to accord your thanks to Colonel Fullerton for his paper. (Applause.)

Mr. FROST: Ladies and gentlemen, we are all very pleased to see Major Baden-Powell in the chair, and I am sure we not only thank him for taking the chair, but we also thank him for the interesting experiments that he has showed us. (Applause.)

This concluded the Meeting.

REVIEW.

"AERODYNAMICS," Vol. I. By F. W. Lanchester. Constable and Co: pp. 442, with many illustrations.

This is a very interesting book, and should be read by all who wish to get a good general idea of the principles of aviation. It must, however, be understood that the book is of a somewhat advanced type; much of the matter contained in it is only likely to be appreciated by those who have made a scientific study of the subject, and the student who takes it up must prepare for some rather tough reading.

The present volume deals only with the theory of aërodynamic support, and the resistance of bodies in motion in a fluid; the questions of equilibrium, stability, etc., being left for discussion in a volume to be published later.

Generally speaking, the first three chapters of the volume now under consideration are devoted to the more elementary principles of fluid dynamics. Chapter IV. deals with certain investigations made by the author on peripteral motion, while chapters V. and VI. give a *résumé* of what is known about aëroplanes. Chapters VII. and VIII. discuss the economics of flight; chapter IX. gives a theory of screw propulsion propounded by the author, and remarks on the power required for flight; while chapter X. concludes the volume with an account of the experiments carried out by Dines, Langley, etc., and also describes certain investigations conducted by the author.

Taking the chapters *seriatim*:—

CHAPTER I. discusses fluid resistance. The Newtonian method of computing it is explained, and the reason why this system, though true enough in itself, fails to be always applicable. The stream-line theory of Rankine and Froude is then considered, and the principles of the transference of energy by the different parts of a stream-line body outlined. The chapter concludes with a brief account of the doctrine of continuity, and notes on stream-line flow in general.

CHAPTER II., on Viscosity and Skin Friction, is a very important one, as the author, like Zahm, maintains strongly that viscosity is the main source of the resistance to which bodies passing through a fluid are subjected. Commencing with Maxwell's definition of viscosity, he explains that skin friction is rather a misleading term, as when a fluid passes the surface of a body a thin film adheres to the surface, and the resistance or drag is more due to the fluid particles rubbing against the particles of the film than to anything in the nature of friction between solids. Various theoretical considerations regarding viscosity follow, and finally the experimental results obtained by Beaufoy, Froude, Dines, and Allen are discussed. It is somewhat surprising that no reference is here made to the work of Zahm, who, curiously enough, is one of the few experimenters in agreement with the author. Zahm's experiments were conducted in air, on lines similar to those of Froude in water, and are certainly deserving of most careful study by all aëronauts.

CHAPTER III.—The Hydrodynamics of Analytical Theory—is a carefully worked out explanation of the theory under consideration. Commencing with the equations of motion for steady flow, the velocity potential, flux function, etc., are commented upon. Ran-

kine's sink and source system is fully described, and some excellent diagrams show the water lines generated by sphere, cylinder, etc. No mention, however, is made of the very ingenious extension of Rankine's theory put forward by Taylor. This experimenter substituted a number of infinitely small sources and sinks for the two or three used by Rankine, and thus got a far greater approximation to ship-shaped forms than any of the earlier workers in this field. The idea is a very good one, especially as by suitably arranging the sources and sinks it seems possible to adapt it to experimental work, and thus ascertain, in a comparatively simple manner, what the real pressures on different parts of bodies and surfaces are.

Some account of Hele-Shaw's experiments might have been given with advantage, as they show the formation of the stream-lines round bodies in a very clear way.

The chapter concludes with a summary of the author's views on discontinuity.

CHAPTER IV. deals with wing forms, and peripteral motion, viz.: the motion of a fluid in the immediate vicinity of a wing. The author rightly notices the great importance of the arched sections and dipping front edges of wings, and points out that these essential features of wing construction seem to have been rather overlooked by most aviators. He gives the credit of their discovery to H. Phillips (1884), who certainly seems to have been the first to point out the rarefaction of the air, *above* certain types of curved surfaces. Lilienthal, of course, described and used curved surfaces, but he did not explain the action of the air in connection with them. The author seems to have formulated a theory of this kind in 1894, but his views were not acceptable to the scientists of the period, and his interesting experiments remained unpublished. The question of aërial support is next considered, and peripteral motion in imaginary and real fluids discussed.

CHAPTER V.—The "Aëroplane"—describes at considerable length the pressures upon normal planes of different types. The results obtained by Hutton, Dines, Langley, etc., are discussed, and the importance of the shape of the planes pointed out. The general normal plane theory is examined, and the chapter closes with a consideration of the question of perforated surfaces.

CHAPTER VI.—The "Inclined Aëroplane—deals with planes of different sizes and shapes, fixed at an angle of inclination to their line of motion. Formulæ for the pressure on them are discussed, especially those of Dines and Langley. No mention, however, is made of Staunton's experiments at the National Physical Laboratory, which is unfortunate, as these experiments are the only ones carried out in this country in which the pressures above and below the atmosphere at different points on plane surfaces have been actually measured.

The very important question of the centre of pressure is only briefly touched upon, but this no doubt will be dealt with in Vol. II.

The chapter ends with a brief account of the author's "Ballasted Aëroplane," and as he uses this apparatus in his skin friction experiments described in Chapter X., it should be carefully studied.

CHAPTER VII. is a theoretical discussion on the "Economics of Flight." The equation of least resistance is examined and the influence of viscosity considered. The practical application of the theory is, however, left for a subsequent chapter.

CHAPTER VIII.—The "Aërofoil," or sustaining part of a flying body. The chapter commences with certain formulæ for finding the best angle of inclination, gliding angle, etc., of sustainers having a wing-like form. As these formulæ contain constants, an effort is made to obtain the approximate value of these latter from experimental results; but it is very sensibly pointed out that the values obtained are "plausible" only, as the present state of our knowledge is not sufficient to enable really accurate results to be obtained.

Various other interesting matters are discussed, such as the best sectional form, etc. What is called by the author "the standard form," viz.: the one used by him in his experiments, is described, and the chapter concludes with a numerical example worked out in detail.

CHAPTER IX.—The Screw Propeller and the power expended in flight. Rankine's well-known formulæ are first examined, and then propulsion in its relation to the body propelled. The various kinds of propelling instruments are mentioned, but the screw propeller is the only one dealt with in this volume. This latter is considered in great detail, efficiency, pressure, distribution on the blades being all discussed. A good many data regarding the marine propeller are given, and there are some rules deduced therefrom for the design of an aërial propeller. Finally, the power required for the flight is examined, and making certain suppositions regarding weight of motor, etc., the author shows that in dynamic flight, speeds exceeding 70 miles an hour are very improbable. In the present state of our knowledge the question of power is a difficult one, but probably when the importance of good shapes of bodies and sustainers is more fully realised the speed of really well designed flying machines will considerably exceed the limits laid down by the author.

CHAPTER X.—"Experimental Aërodynamics"—is a very interesting one, as it deals very fully with the methods employed by various experimenters. Commencing with Hutton and the older workers in this field, the author gives details of the whirling tables, etc., used by them. Dines' method is next described and his results considered, and a remarkable innovation, the triangular (in section) surfaces used by him commented upon. The author thinks (see page 347) that this peculiar shape would not invalidate the results obtained at the higher angles of inclination, but he is doubtful as to the effect at the lower inclinations. It would certainly seem that Dines' results for small angles require carefully checking with the work of other experimenters, as there is no doubt that at small angles of inclination the form of the surface under test has a very great influence on the air pressure.

A very detailed account of Langley's experiments is given, and the author does not, generally speaking, admit them to be satisfactory. He points out various defects in the manner of drawing the smooth curve, of the resultant pressure recorder, the doubtful error, said by Langley to be due to the bending of the arm of the whirling table, etc., and especially objects to the conclusion drawn, that skin friction is a negligible quantity.

Before proceeding to describe his own experiments, the author makes one very important statement, viz.: that the equilibrium of a bird in flight is *not* dependent upon the intervention of the brain and nerve centres, and that it is *not* necessary to supply some "brain equivalent" to be directed by the aëronaut. There is no doubt that this view is correct, and that, as stated by the author, "a properly-designed rigid structure is capable of maintaining its own equilibrium, and possesses complete stability within pre-arranged limits.

A short account of the author's own experiments on the value of the skin friction co-efficient follows, but it must be confessed that they are not very convincing. He used three different systems for his tests, viz.: the "added surface," the "total surface," and the "ballasted aëroplane" device described above. A good account of the method of carrying out the tests is given, and the tables and calculations furnish a certain amount of useful information. The great objections, however, to all the methods are the defective arrangements for starting the models and the want of accurate instruments for automatically recording the time of flight, path, etc. It is impossible to get satisfactory results by throwing the aëroplanes, and the taking of times by a stop watch is not nearly accurate enough for scientific work, particularly as such very small models were used.

The experiments were, however, interesting, and would at all events throw some light on the equilibrium question, a matter of very great importance.

The work concludes with some appendices and a very good index. The excellent illustrations are an important feature of the book.

NOTES.

Retirement of Col. J. L. Templer, late Superintendent Royal Engineers' Balloon Factory, Aldershot, Member of Council of the Aëronautical Society of Great Britain.—The following extract from the "Daily Telegraph," will no doubt be read with interest by the Members of the Society:—

To-day Colonel James L. Templer, formerly of the King's Royal Rifles, retires from the distinguished official position he has so long and so admirably filled under the War Office in connection with the Royal Engineers' balloon establishment at Aldershot. Somewhere about April 1 seems to be a chosen time for important military manœuvres. It will be recalled that some little time ago Colonel Templer was superintendent of the balloon factory, the work of which also included the construction of steam road-transport engines, as well as everything in connection with Army aëronautics, and that he held the post for many years. Latterly he has been the official technical adviser to that highly-specialised department, a duty no one was better qualified to fill than the gallant Colonel, who has seen service at home and abroad, in peace and in war. The "age clause" brought about his first severance, and now he ceases altogether the official connection. It will not be easy, if it is even possible, to replace the gentleman who through weary years has untiringly wrought to keep the War Office interested in aëronautics, and done so much to secure for the Army the best and most complete balloon and signalling equipment of any service in any country.

Colonel Templer, although of tall, athletic build, took in the first instance to ballooning as a pastime and sport. He made many ascents with Mr. T. Wright, a pupil of Mr. Coxwell's, as did the late Colonel Fred Burnaby and other well-known personages. Impressed with the possibilities of aërial navigation, the Colonel determined to make a scientific study of the whole subject.

AN AËRIAL ENTHUSIAST.

He saw at once that it could be made of great use in many ways in military operations. So, in 1877, he put forward his plans to the War Department, and was appointed as an assistant to Sir Charles Watson, K.C.M.G. From that time forward he has been in the Service, and become famous everywhere, and is perhaps the best-known aëronaut, at least among experts, in any country. He initiated and perfected the manufacture of goldbeaters' skin balloons, the best receptacles for holding hydrogen gas. And in this direction the public owe to him new methods for making, compressing, and storing the gas for military ballooning. Colonel Templer made endless experiments in these and other branches bearing upon aëronautics, and he devised various apparatus for gauging wind-pressures, currents, as well as types for captive, free, and dirigible balloons. Even aëroplanes were not neglected, and years ago, before and after Sir Hiram Maxim's experiments to solve flight, Colonel Templer laboured in the same direction. Upon the Continent to-day, his tables and methods are the accepted standards by which officials and private persons conduct their operations in the domain of aëronautics. The British public will remember him best as the official who six years ago believed in the dirigible balloon, and obtained the consent of none too willing authorities to build "Dirigible No. 1" and No. 2. It was the first in which, last year, Colonel Capper and Mr. Cody came screwing up from Aldershot to London, and alighted within the Crystal Palace grounds.

BALLOONS AND AËROPLANES.

Very early in the pursuit of his favourite study the enthusiastic aëronaut decided that coal gas was only a little less bulky and out of date for balloons than the hot air used by the Montgolfiers. So he set about manufacturing skin balloons and using hydrogen for their inflation. And this is an instance in which figures speak volumes. The weight, say, in vacua of 1,000 cubic feet of ordinary air is at a barometer pressure of 30 in. and 50 deg. Fahrenheit temperature about 75 lb. Now 1,000 cubic feet of coal gas weighs from 35 lbs. to 40 lbs., whereas the same quantity of ordinary hydrogen weighs not more than 5 lb. Were the hydrogen absolutely pure it might weigh but a matter of 2 lb. per 1,000 cubic feet. The difference between the 75 lb. and the weight of coal gas or hydrogen, plus the material for the balloon, gives the lifting power exerted. Colonel Templer made in 1884 a skin balloon with a capacity of 4,500 cubic feet. It has been used since then in making hundreds of ascents, and is still in good condition, able, when filled with hydrogen, to lift a man of medium weight. Since his entry into official life Colonel Templer has had built nearly 1,000 balloons of various kinds for the Army. He has built the first dirigible military balloon, has seen the birth and growth of kite flying for war purposes, and has with his own hands devised and schemed to construct practical aëroplanes. From the beginning he has maintained the practical utility of the dirigible balloon, even with existing means, and he has said that the aëroplane only awaits the coming of the suitable engine, or turbine, to make it a success. He has only held his hand of late with the aëroplane whilst he was seeking to perfect a suitable engine. We are, he declares, at the dawn of the era when aërial navigation will compete with the road motors. And Colonel Templer does not mean to discontinue his study and services to the cause of aërial navigation.

Lecture on Aërial Navigation, by Mr. H. Chatley, B.Sc.—An interesting lecture on the subject of aërial navigation was given by Mr. Chatley, at a meeting of the Junior Institution of Engineers, on February 7, 1908. The lecturer described the various types of aërial machines, but dealt more especially with aëroplanes, partly, as he stated, because a thorough understanding of their principles is necessary before other types can be rightly considered, and also because the most interesting results have so far been obtained from machines of this class.

The lecture was attended by many aëronauts, and in the discussion which took place afterwards, some very interesting information was given by Mons. Julliot about the French dirigible balloons, and by Capt. Ferber about his own aëroplane experiments. The lecture is published in the "Automotor" of February 15, 1908.

Cost of British Military Airships.—According to the Army Appropriation Accounts for 1906-7, the expenditure on "Nulli Secundus" up to March 31, 1907, amounted to £6,679 4s. 9d. Other interesting figures given are the sum of £9,091 9s. 6d., representing the value of articles manufactured and services performed at the balloon factory, which compares with the sum of £8,810 6s. 2d. for the year 1905-6.—"Automotor."

The Cordingley Motor Exhibition (Aëronautical Section).—This exhibition was held at the Agricultural Hall from March 21 to 28, and although the aëronautical exhibits were unfortunately by no means numerous, some interesting machines were on view. Messrs. Spencer Bros. showed various types of spherical balloons, and two

full-sized cars, with motor and propellers complete, for use with dirigibles. They also had a very interesting collection of photographs, prints, etc. T. W. Clarke and Co. had a number of models of the Langley type on view, and the demonstration of the flying capabilities of the smaller ones was quite one of the features of the exhibition. One of the larger ones, which weighed 1.4 lbs., with an area of 3.1 sq. ft., had flown 400 feet in the open air, the motor used with it being 14 ozs. of twisted rubber.

Mr. Stanger's aëroplane was also an interesting one. Its sustainer consisted of one top and two bottom surfaces, the average spread being 7½ ft., while the average width was 3 ft. Total weight in order of march, 20 lbs. The motor weighed 8 lbs., and gave 1½-h.p. There were 4 cylinders, 1¼-in. bore, 1½-in. stroke, with 50 lbs. on the sq. in. compression. At 15 miles per hour the revolutions were 1,300 per minute. The best flight made was 200 yards (stopped by a tree), and the machine on that occasion rose from the ground between 40 and 50 yards from the starting point.

Of *light motors*, some interesting specimens were shown. That of Mr. Lambert weighed 15 lbs., and gave off 3.2-h.p, by R.A.C. rating, while two J.A.P. motors, weighing 53 lbs. for 8-h.p. and 130 lbs. for 16-h.p., showed that the question of providing motors suitable for aërial work is now attracting the attention of engineers in this country.

The aëronautical section was in charge of Messrs. Spencer Bros., who must be congratulated on the success of their efforts to make it interesting.

Mr. E. Wilson's Aëroplane.—Mr. Wilson has now nearly completed a full-sized machine, and hopes to be able to commence its trials very shortly. It is of the Farman-Delagrange type; main planes, 32 ft. by 6½ ft., spaced 6 ft. apart, arching hyperbolic, posterior margins, flexible. The box-kite front elevating planes are 10 ft. by 3 ft., while the tail in rear is of a similar type, 8 ft. by 5 ft. Total sustaining area = 528 sq. ft. The total weight in order of march is 600 lbs; which includes operator, a 16-h.p. engine, two hours' supply of fuel, and a pair of gyrostats (30 lbs. each), to facilitate lateral control. The propellers are twin screw, 6½ ft. in diameter, with variable pitch. Longitudinal stability is secured by the length of the machine (32 ft. all over), together with automatic adjustment of the front planes by operators' weight acting as a pendulum to counteract diving.

"La Patrie."—A very interesting detailed account of this dirigible is given by Chef de Bataillon Voyer, Corps du Génie, in the "Revue du Génie Militaire" for February, 1908. As is well known, the general design of this airship was the same as that of the "Lebaudy"; but in minor details there was really a good deal of difference. The author describes these differences very clearly, and also gives an account of the principal journeys made by "La Patrie," including the last one, when it was lost. He sums up the general results of the numerous trials as follows:—

1st.—The velocity of a dirigible of this type, though it has not been measured very exactly, is about 40 kilm. (24.8 miles) per hour, in still air.

2nd.—With an opposing wind of about 7 m. per sec. (15.5 miles per hour), one can count on a velocity of at least 30 kilm. (18.6 miles) per hour. In a 10 hours' run a distance of 300 kilm. (186 miles) might be traversed, the radius action being 150 kilm. (93 miles).

3rd.—The amount of ballast carried by this type and the capacity of its "ballonet," allow of an altitude of 1,500 m. (4921 ft.), with four aëronauts in the car.

The article is remarkably well illustrated, as there are twenty-three photographs showing the balloon in motion, entering and leaving its shed, etc.

Model Dirigible at the Surrey Masonic Hall.—Through the kindness of Mr. Whitby, members of the Society were able to see this model in action on January 24.

The envelope was 14 ft. long, 3½ ft. in maximum diameter, and held about 100 cubic feet of hydrogen. The inventor's description of his machine is as follows:—

"It is an entirely original, exceedingly simple, and very effective contrivance for aërial navigation. It has the power of raising an airship perpendicularly, or at any desired angle, without discharging ballast; lowering without releasing gas; propelling forwards or backwards with considerable speed; steering perfectly; coming to a standstill in mid-air; and turning in about its own length, without stopping or interfering with the motor for either of these manœuvres, which are all accomplished by a very quick and simple means. There are no "wings," gear-wheels, or any delicate attachments whatever, to require attention, add to the weight to be carried, or to get out of working order; and above all, *no rudder is required.*

The trials made before the visitors appeared to be satisfactory; the balloon moved from one side of the hall to the other, rose in a slanting direction and perpendicularly, and performed various manœuvres. The spectators were not allowed to examine the machine as the inventor desires to keep the arrange-

ment for steering, etc., secret, so that it is difficult to give an opinion of its merits, but as far as could be seen the model did what was claimed for it.

Accident to the Gastambide-Mengin Aëroplane.—This machine is of great interest, as it is one of the very few that is almost an exact imitation of a bird. The "New York Herald" gives the following account of the accident:—

The Gastambide-Mengin aëroplane met with a serious accident at Bagatelle, in the Bois de Boulogne, yesterday morning, and, though M. Boyen, the driver, was not injured, the apparatus was considerably damaged. The machine has been out on several occasions of late, and, as is usual, the preparations and evolutions were watched by a crowd of interested spectators. A preliminary flight of fifty yards or so was made with success about eleven a.m., and as the motor was working regularly even better things were anticipated. The aëroplane was then taken to the Longchamps end of the field, and as soon as the propeller had been set in motion the apparatus dashed off towards Neuilly. After running along the ground for about a hundred mètres the plane lifted, and swaying considerably to right and left and varying its height every second, it rushed through the air for 150 mètres or thereabouts.

All at once the apparatus was seen to dive sideways and fall fast. The propeller and bow of the aëroplane cut into the earth, causing it to turn a somersault, and as it fell on its back there was a loud noise of rending timber. Spectators rushed up, expecting to find the driver crushed beneath the heavy frame and motor. Fortunately M. Boyen had seen the danger and had crouched into the well behind the motor. When the crash came he was sheltered from all harm and was able to crawl out between the ribs, to the amazement of the bystanders.

The machine was badly damaged, and will require considerable repairs. The wheels were twisted up and the propeller reduced to a tangled strip of iron. The tail was dislocated and the wing supports snapped.

The general impression concerning the Gastambide aëroplane is that similar accidents will happen regularly as long as the constructors refuse to fit a horizontal rudder or headpiece, whereby the height and angle of the machine may be controlled when in the air. With the aëroplane as it was yesterday, once the ground had been left, the only means possessed by the driver to control upward and downward movement was the motor.

Aëroplanes are hardly sufficiently advanced yet to rely upon the suppleness of a petrol motor for angle and height. The most skilful aëroplane driver finds he has enough to do in controlling the general direction of the apparatus by simple means, without having to trouble about the ignition and carburation of his motor.

It was further remarked that, however sound in theory the single plane aëroplane may be, every serious accident yet recorded has occurred with this type. M. Blériot, who was an interested spectator of yesterday's accident, has had three falls with his Langley flyer, M. Esnault-Pelterie, one or two, Comte Henry de La Vaulx, one at least, and now M. Boyen, with the Gastambide-Mengin aëroplane.

On the other hand, Mr. Henry Farman never had an accident of any importance with his cellular plane during the 250 flights accomplished. M. Delagrange also has escaped with one fall, due to the breaking of some wires. The movements in the air of all single plane machines appear to be too rapid for the beginner. This, at any rate, is the opinion of such authorities as Captain Ferber and Mr. Farman. To learn to fly, slow-moving types of aëroplane are essential. When the knack has been acquired, swifter and less stable systems may become possible.

The New French Army Dirigible.—According to the "Practical Engineer," the new airships that are to be built for the French Army, from the design of M. Julliot, are to be nearly twice the size of the "La Patrie," the balloon having a capacity of from 25,000 to 26,000 cubic feet of gas. Their length will be 328 ft., the greatest transverse diameter 37¾ ft., and, like all French airships, they will have an interior compensating air balloon to ensure rigidity of the outer envelope, which will be of double-lined guttapercha. There will be two 120-horse power motors, each driving two propellers, one pair being placed forward and one aft, and capable of producing a speed of thirty-five miles an hour, which, with the large lifting power of the balloon, and its ability for carrying a large quantity of provisions, petrol, etc., will enable the vessel to remain in the air for a prolonged period, and be a really formidable auxiliary of the army.

Mr. Horatio Phillips's New Machine.—This machine, which is now being experimented with, is described in "Engineering" for February 28, 1908. The sustainers are of the usual Phillips type, but in order to improve the stability, there are four of them instead of the single sustainer used in the earlier machines. The motor is an 8-cylinder air-cooled one of Mr. Phillips' own invention, and gives 20-22 brake h.p. at 1,200 revolutions per minute.

It is hoped to publish fuller details of this machine in a later issue of the Journal.

Foreign Aëronautical Publications.

(In this list a selection of some of the more notable articles is only given.)

L'Aérophile.

January 1, 1908.—Bazin's Aëroplane, Beating Wings Type.—The Second Voyage of the "Ziegler" from Germany to England.

January 13, 1908.—Winning of the Deutsch-Archdeacon Prize by H. Farman.—The Journey of the "Ville de Paris" to Verdun.—From Rome to the Adriatic by Balloon.

February 1, 1908.—The Banquet to H. Farman.—Journeys Made by the "Ville de Paris," with Description of the Balloon.

February 15, 1908.—The Antoinette Motor.—H. Farman's Report to the Academy of Science.—The Efficiency of Screws in Air.—Capt. Ferber and Mons. Julliot at the Junior Institution of Engineers.

March 1, 1908.—The Elrich-Wels Flying Machine.—On the Pitching of Aëroplanes.—The Ellehammer Aëroplane.—The Gastambide-Mengin Aëroplane.

March 15, 1908.—Artillery Fire Against Dirigible Balloons.—The "Flying Fish."—Birds, Aëroplanes, and the Co-Efficient of Air Resistance.—The Aëronautical Meeting at Bordeaux.—The Gilbert Aëroplane.

L'Aéronaute.

November, 1907.—Address by Mons. Armengaud.—Study of a Propeller.—The Farcot Motor.

The American Aeronaut.

January, 1908.—The Perfect Flying Machine; A Description of the Wright Bros'. Machine.—Zeppelin's Triumph.—The Government Flying Machine.

Aeronautics.

January, 1908.—The Government Dirigible and Dynamic Flyer.—The California Arrow.—Speed of American Dirigibles at St. Louis.

February, 1908.—Farman and the Grand Prix.—Experiments with Model Flying Machines, by E. W. Smith.

March, 1908.—The Value of the Motorless Glider.—"Curvature," a Relative Term, by G. A. Spratt.

February-March, 1908.—Motor Ballooning. Farman's Triumph.—The Use of Air Currents in Ballooning.—Airship Engines.

Bollettino della Societa Aeronautica Italiana.

No. 1.—On Air Resistance.—Aërological Experiments in July, 1907.—Aviation, with Accounts of Various Flying Machines.—Dirigible Balloons.

No. 2.—Notes on the Autorotation of Symmetrical Plates in a Current of Air or Water.—The Winds in Italy.—Aviation.—Dirigible Balloons.—Scientific Chronicle.

No. 3.—Aërodynamics and Aviation (A Review of Mr. Lanchester's Work).—Visits to the Aërological Observatories of Lindenburg and Strasburg.—Aviation.—Dirigible Balloons.—Scientific Chronicle.—Sporting Supplement.

Illustrierte Aeronautische Mitteilungen.

January 3, 1908.—The Stability of Flying Machines.—Count Zeppelin.—The Jatho Flying Machine.

January 18, 1908.—The Loss of "La Patrie."—Miscellaneous Flying Machines.

February 3, 1908.—The Farman Success.—The "Wellin" Flying Machine.—Airship Motors: the "Antoinette."—Miscellaneous Notes.

February 18, 1908.—The Stability of Flying Apparatus.—The "Jatho" Flying Machine.—The First English Military Airship.

March 3, 1908.—The Stability of Flying Apparatus.—Miscellaneous Notes.

March 18, 1908.—The Stability of Flying Apparatus.—Miscellaneous Notes.

Wiener Luftschiffer-Zeitung.

January, 1908.—Airships in 1907.—Pierre Janssen.—The Loss of the "Patrie."—Aëronautical Maps.—Chanute and Etrich-Wels.

February, 1908.—Janssen.—The "Deutsch" Balloon; From Paris to Verdun.—Farman's Flight.

March, 1908.—About the "Zeppelin" Balloon.—A Visit to the Bros. Voisin.

Applications for Patents.

(*Made in January, February and March.*)

The following list of Applications for Patents connected with Aëronautics has been specially compiled for the Aëronautical Journal by Messrs. Bromhead & Co, Patent Agents, 33, Cannon Street, London, E.C.

JANUARY.

84. January 1st. E. Neyen. **Improvements in steerable aerodynamic airships.**

221. January 3rd. R. Esnault-Pelterie. **Improvements in aeroplanes.**

528. January 9th. R. B. Woosnam. **Improvements in and relating to flying machines without aerostats.**

589. January 10th. J. Fozer and I. Foot. Improvements in airships.

1035. January 16th. J. L. Garsed. Improvements in or connected with aerial machines.

1258. January 18th. R. Esnault-Pelterie. Improvements in aeroplanes or flying machines.

1307. January 20th. A. Wunderlich. Improvements in or relating to motor flying machines.

1438. January 21st. A. Sainte-Beuve. Improvements in and relating to sheds and like structures for housing balloons and other purposes.

1449. January 21st. H. F. Phillips. Improvements in or relating to flying machines.

1569. January 22nd. W. I. Wilson. Apparatus or device for producing currents in fluids or acting inversely as a fluid current motor applicable for marine, submarine, and aerial propulsion and steering and for pumping, excavating, lifting, dredging and the like.

1593. January 23rd. M. Ferrero. Improvements in or relating to apparatus for aerial navigation.

1683. January 24th. O. Heeren. Improvement in aeroplanes.

FEBRUARY.

2493. February 4th. J. Wilson. Improvements in Airships.

2588. February 5th. L. Bleriot Improvements in or relating to balancing and steering apparatus.

2651. February 6th. W. P. Thompson. Improvements in flying machines.

2793. February 7th. D. F. Shearer. Improvements in or relating to flying machines.

2808. February 7th J. W. Dunne and A. K. Huntington. Improvements in and relating to aeroplanes.

2907. February 10th. W. F. Howard. Improvements in aeroplanes.

3029. February 11th. F. J. Alderson. Improved motive power for aeroplanes, balloons, and war projectiles.

3479. February 15th. N. Blood. Improvements in or relating to flying machines.

3538. February 17th. H. L. Todd. Improvements in airships.

3909. February 20th. H. Goodacre. Improvements in aeroplanes.

4054. February 22nd. H. H. Cooper. Improvements in or relating to the propelling apparatus of balloons, airships, and the like.

4055. February 22nd. J. G. H. Batchelor. Improvements in propellers and other bladed apparatus of balloons, airships, and the like.

4519. February 28th. A. H. Edwards. Improvements in or relating to aeroplanes or the like, or analogous machines or devices.

MARCH.

4677. March 2nd. J. Westaway. Improvements in or connected with aeronautical machines.

4769. March 3rd. S. H. Hollands. Improvements in aerial propellers and air-propelling screw fans.

4788. March 3rd. Motorluftschiff-Studienges. Improvements in a non-rigid air screw with fly weights.

4789. March 3rd. Motorluftschiff-Studienges. Improvements in a process and device for regulating the inclination of an oblong aerostat or airship by means of two air bags.

4813. March 3rd. C. Davis Improvements in means for use with balloons and other air vessels for indicating air currents.

4842. March 3rd. E. Von. Bernd. Improvements in wheels with rotary vanes suitable for airships, flying apparatus, and the like.

5193. March 7th. J. R. Porter. Improvements in propellers for use in air or water.

5220. March 7th. E. J. Lester and W. G. Beat. Improvements in and relating to airships and aeroplanes.

5310. March 9th. J. Chantraine. Improvements in flying machines.

5312. March 9th. S. Y. Beath and G. Whitzhead. Improvements in aeroplanes.

5471. March 11th. G. Kerwat. Improvements in airships.

5508. March 11th. E. P. Alabaster. Improvements in aerial flotation.

5826. March 16th. G. Brewer. Improvements in grapnels, particularly applicable for use with aerostats.

5949. March 17th. A. Mutti and R. L. Mond. Improvements relating to apparatus for propelling vessels in air or water.

5977. March 17th. J. Cervelli, J. Molinari and J. Bernasconi. Improved mechanism designed to be employed for the propulsion of ships, submarines, and aerostats as well as for the production of motive power by means of a current of air or water.

6159. March 19th. H. F. Phillips. Improvements in flying machines.